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THE

JOURNAL OF SCIENCE.

JANUARY, 1882.

I. THE LAW OF EVOLUTION: IS IT GENERAL?

THE truth of Evolution as a primary law of the Universe must to a great extent turn on its generality. If we see traces of its action—not demonstrable, but merely probable—in each class of phenomena which we successively examine, the difficulties in the way of its acceptance will be very much lessened. Now we find it beyond all dispute in the growth of the individual animal or vegetable from the germ to maturity. We say "beyond dispute," though we know a somewhat eccentric thinker who maintains that this development is an illusion, and that the infant, the boy, the youth, and the man are not four successive stages of one and the same being, but four distinct beings which succeed each other. Whether he puts forward this strange speculation as a mere dialectical exercise, or as a *reductio ad absurdum* of the hypothesis of separate creation, we have never been able to feel satisfied.

To return: we recognise Evolution, then, in the individual, in the nation, and in human society at large. In the animal and vegetable world we see good cause to accept it as the probable method in which those forms we call species have originated. Passing on to the other extremity of the scale, Evolution, as opposed to individual appearance, seems generally received as the process by which the heavenly bodies have taken their rise. It is not for this purpose necessary that we should accept the "nebular hypothesis," the cosmogony of Kant and La Place, and deduce suns and planets from the gradual condensation of a fire mist. The obvious difficulties which beset this hypothesis need not, as modern

astronomers have shown, compel us to the old doctrines of eternal existence or of mechanical contract-work.

But between the vital and the astronomical lies a very important region, to which, in the opinion of many men of Science, the doctrines of Evolution are inapplicable. We refer to the region of chemical phenomena. If this vast and important sphere shows no signs of the operations of Evolution,—still more if it gives proof of their absence,—then we have to do not with any universal principle, but with a possibly exceptional agency. We shall have no *à priori* right for assuming its presence and intervention where not fully demonstrated.

We turn first to the so-called chemical elements, and ask what is their origin? Have they been developed, as we suppose animal and vegetable species to have been, from a few kinds of primordial matter, or even from one kind only? Have they existed from all eternity as the raw materials of the universe, capable of being associated chemically into compounds and mechanically into aggregates, varying in bulk and importance from the dust-grain to the star? Or have they been called into existence independently of each other, whether by some external power—self-conscious or otherwise—or by some unknown impulse inherent in matter? To none of these questions does Science return a definite answer; she offers us probabilities, indeed, but not certainties. The physicist is able to tell us that suns and earths, as such, cannot have existed for ever; they have had a beginning, and must have an end. The universe, think some, is one huge grave-yard for dead stars. But all this throws no light upon the origin of the chemical elements. Certain metals, gases, and the like exist in the Sun, in Sirius, or in Vega; but this does not tell us whether the same particles of matter first sprang into existence with the orbs they help to constitute, or whether they have previously been present in stars now no longer in existence. The chemist tells us that neither by the aid of the spectroscope nor yet by the analysis of meteoric stones which arrive from the depths of space can he find indications of any other kinds of matter than the sixty odd elements which are met with in our own planet. But this conclusion, however interesting in itself, is of no direct use for our present purpose. Assuming it is proved that the whole universe is built up of these sixty, we must pronounce that assumption equally consistent with the hypothesis of eternal existence, of special origin (creation in a mechanical sense), or of evolution.

The decomposition of any of the supposed elements and

their resolution into simpler primitive forms of matter, would of course have been decisive; but this decomposition has not been effected. The experiments of Profs. Victor and Carl Meyer on the vapour-densities of the halogens at high temperatures, and the spectroscopic observations of Mr. Norman Lockyer on the sun and on certain of the more brilliant stars, have given rise to hopes. But all of the results obtained admit of other interpretations than that of the decomposition or approaching decomposition of an elementary body. It must, indeed, be admitted that the prospect of such a decomposition is not becoming stronger. If we consider the legions of chemists who are at work in the present day, and the improved resources at their command, we cannot avoid a suspicion that if the compound nature of the elements were capable of experimental demonstration some definite advance towards that end would have been already made.

We must therefore search for indirect evidence. Here, then, we find not a few circumstances which militate, apparently at least, against evolution and continuity. We see, in chemical phenomena, as Mr. Baildon has ably pointed out, not gradual serial change, but abrupt transition. The various elements do not pass into each other by imperceptible changes, but, with few exceptions, they display well-marked contrasts. No one can assert that, like organic species and varieties, they differ merely subjectively. We have not in chemistry, as in botany or zoology, contests between "lumpers" and "splitters" as to the number of distinct forms in existence.

Again, if we study what takes place in the act of chemical combination, we find no gradual passage through intermediate stages. We may have, *a*, a mixture of oxygen and hydrogen, and *b*, water, formed by the combination of these two elements. But no one has detected any intermediate phase, any transition between fully-formed water and its constituents. Chemical compounds—those, at least, of a lower order—pass through no embryonic condition.

It may even be asked whether the law of combination in certain fixed proportions, and in their multiples only, is in harmony with the fundamental ideas of evolution and continuity? We have just seen that in the genesis of water Nature does act *per saltum*, and the same holds good with other bodies of a similar rank.

But we trace in the chemical region other considerations which tell very strongly in favour of evolution. We find among the elements certain which are exceedingly abundant,

such as aluminium, silicon, iron, oxygen, sodium, &c. : others comparatively rare, such as indium, vanadium, thallium, iridium, &c. How is this ? On the hypothesis of primordial existence such a distinction is not easily to be explained. On the evolutionary view it may be fairly contended that the circumstances under which the rarer elements have been produced may have been of unfrequent occurrence as compared with those under which the common simple bodies were developed. To the objection that we never find any elementary substances changing their nature, or reverting into the primordial state of matter,—an objection which reminds us of the cavil that we never see a man reverting to an ape,—it may be replied that under given conditions matter has reached forms which are stable, and that none of the original "raw material" remains. We may here refer to the view expressed by the late Prof. Clerk Maxwell, and by Sir John Herschell, that the elementary atoms bear the impress of a "manufactured article." But every manufactured article presupposes an original material. Thus this hypothesis agrees in its requirements with the evolutionist view, and the impossibility of producing a specimen of the "raw material" is of little weight against either.

But let us now suppose some sixty kinds of elemental bodies, some of them more and some of them less plentiful, either co-existing from all eternity or called into independent existence. Were all these bodies indifferent to each other the probability is surely that the atoms of each kind would be distributed with approximate uniformity, regard only being had to their relative abundance. If, as is actually the case, they are not indifferent, but have those various tendencies to combine together which are commonly called affinities, we should find those bodies accumulated together which have the strongest affinities for each other, whilst those which have but feeble mutual affinities, or substantially none, would be found mutually apart. Now as a general rule it may be asserted that those elements which have strong affinities for each other are at the same time such as differ widely in their properties, and that, on the contrary, such as approximate closely in their properties have little mutual affinity. But if we examine into the composition of the earth, the rocks, and the waters, we very commonly find bodies associated together which are very similar to each other, and which have very feeble mutual affinities. To take a very common case, we find in the waters of the sea, of certain lakes, and springs, the three true halogens—chlorine bromine, and iodine—associated together, the two latter

always in exceedingly minute proportions as compared with the former. Now these three substances have such very doubtful affinities for each other that, if primordially existing or created separately, we cannot reasonably suppose them becoming grouped together as they now occur. But if they have been developed from some common primary material, or if bromine and iodine are transformation-products of chlorine, their joint occurrence is what we should naturally expect. Or, again, we may take the case of nickel and cobalt. These metals, as a rule, accompany each other in Nature. Wherefore? The teleologist has nothing to urge; for even if we grant that man is the object and measure of all things, we remember that these two metals and their respective combinations are rendered of much less value to man by being mixed up together. It may be said that the very considerable similarity of these two metals in their physical and chemical properties opposes every natural agency for their separation, just as it obstructs and delays the analyst and the metallurgist in the same operation. We grant the difficulty of separating them when once in association; but what should first bring into such general juxtaposition two kinds of bodies, not over-abundant, if there has ever been a grand game of hide and seek played by the elements? But on the supposition of development the case is easily understood. There are other groups of elements which in like manner are met with together in Nature, and which certainly do not manifest any especial mutual attraction. Thus there are the platinum metals, and — most striking case of all—those recently discovered elements which have been obtained from samarskite. These bodies are not only exceedingly rare, and confined to a very few localities (as regards chemical elements the particular rock or mineral in which they occur seems to take the place of the *habitat* of organic species), but they differ so slightly in their properties that we cannot help regarding them as varieties formed by a process of development.

The periodic law of Prof. Mendelejeff, to which successive researches seem to give confirmation, can scarcely be interpreted as otherwise than in favour of the evolution of the elements from some common fundamental material. If primordially existing it seems to us that the probability of regular relations being detected among them would be exceedingly trifling. Nor do such relations* become at all more probable if regarded from a teleological point of view.

* Journal of Science, 1877, p. 291.

But if we look at a table of the elements, and consider their properties, we can scarcely fail to be reminded of the features presented by a group of organic forms. In such a group—say in a large genus of insects—we find a few species common, numerous, and well-marked. We then see a number of others comparatively scarce, or indeed very rare. Among these rare species we find several which are difficult to distinguish from each other, and we often trace sub-groups inhabiting the same district, and having an especially strong family likeness. We find species standing widely apart from each other, and between which some connecting-link seems either to have disappeared or to have escaped notice, and others again so closely similar that the discovery of any further allied species would be felt as an inconvenience. Again, if we try to arrange animal forms in any regular series, according as they depart gradually from some given type, we proceed for a while very satisfactorily; but before long we come to a species which seems equally well entitled to two or even more positions.

If a digression is here permissible we may ask whether something like periodicity does not obtain in the organic world, and whether an indistinct recognition of this principle —overlaid indeed with assumptions and errors in abundance —was not the grain of truth hidden in the "quinarian" or representative system of Macleay, Swainson, and their allies? Naturalists who consider that species, like individuals, are evolved not at haphazard, but in certain given directions, will perhaps see here a clue to an advance in taxonomy.*

But to return: these very features which we have just traced—say in the Coleopterous genus *Aphodius*—may be in the main recognised in our table of chemical elements. Here, again, we find a few common species and many rare ones, the latter generally clinging together, being often difficult to discriminate, and inhabiting some special geological formation or some mineral species, just as a zoological subgenus is found in some island like Macassar or Madagascar. We do not, indeed, find among the elements any two whose distinct nature remains for a length of time doubtful. But we come upon difficulties of classification very similar to those encountered in the organic world, aberrant and anomalous members which have not been fully reduced to order even by the periodic classification.

We have pointed out the abruptness of chemical combi-

* We shall probably, by opportunity, resume this question.

nation and the sudden and complete change which ensues when elementary bodies unite together to form some compound; but it may, even in this respect, be submitted that what in the simpler inorganic bodies appears so discontinuous is greatly modified in the higher organic compounds. In such bodies as water, ammonia, the anhydrides, the metallic oxides, &c., we find two elements only present, a single atom of one of these being united to one, two, three, or perhaps four of the other, forming distinct stages between which there is nothing intermediate. But in organic compounds the combining weight of each element has often to be multiplied by some very high number, and thus the series becomes continuous, the law of definite proportions notwithstanding. We have said above that the lower inorganic kind of chemical compounds have no embryonic intermediate state. But as the higher organic compounds appear to be formed not by the union of distinct and simple atoms, but of groups of atoms which are in themselves compound, it may be asked whether these proximate constituents do not in some degree stand in the stead of an embryonic state?

We have referred to the absence of an intermediate stage in such cases as the formation of water; but the study of living bodies supplies us with a transformation no less sudden. Matter is either living or lifeless; there is no intermediate third stage. A particle of lifeless inorganic matter brought into contact, under certain circumstances, with the organised body flashes into life. The "merorganic" stage of certain chemists and physiologists of the second quarter of the present century is no longer recognisable. Comparing, then, the two pictures, the table of the elements, and the general features of chemical phenomena, on the one hand, and on the other the ranks of the organic kingdoms and the laws of vital phenomena, we see no such decided distinction as would forbid us from pronouncing the former as situate altogether outside the scope of the law of Evolution. And if we pass from the simple elements to their compounds actually existing upon the earth, we find that the analogy becomes closer,—that we may even trace some of the workings of the principle of Natural Selection. This will appear as follows:—The elements have, we believe, varying tendencies to combine among themselves, just as organisms have inborn tendencies to modification in certain directions. These tendencies are, however, exerted under varying conditions of temperature, pressure, mutual proportion, access or non-access of air and moisture, and presence or absence in each case of substances capable of interfering

or modifying the result. Hence among the compounds formed those which are in harmony with surrounding conditions are preserved, whilst those not in such harmony are decomposed and give place for others. Thus the struggle for existence is being waged among mineral bodies, governed by purely chemical and physical forces as it is among animals and plants, where, in addition, we recognise an unknown agency which we call "life." Whether this life is merely a modification of the ordinary chemical and physical energies working under a new set of conditions, or whether it is a totally distinct kind of energy, or whether, lastly, it is due to the presence in the plant and the animal of some immaterial power, is for our present purpose beside the question.

It appears, then, that chemical phenomena do not present a region within which the great principle of Evolution does not hold good. That it acts—necessarily acts—here in a manner different in some respects from what is observed in the organic world need not perplex us, if we do not insist upon pushing too far the analogies between dead matter and living, growing, reproductive organisms, whose existence is not linear and of undeterminate extent, but cyclical or periodic.

II. DISUNION IN THE CAMP.

IN opposing a fanatical movement such as the Anti-Vivisection agitation there can be surely no doubt as to the absolute necessity for union among the friends of Science. We must be agreed as to our position and our demands, and there must be no attempts to make terms with the utterly irrational and irreconcilable enemy before us. Our position is this: that if it be justifiable to inflict pain and death upon animals for gain, for amusement, for convenience, for food, or for self-defence, it is, to say the least, equally justifiable to inflict pain and death in pursuit of knowledge. Our demands are the entire and unconditional repeal of the Vivisection Act, and the concession of the same freedom of action to the biologist as he formerly

enjoyed, and as is now possessed by the chemist and the physicist. If we take up any lower ground,—if we accept any compromise, any circumscribed liberty trammelled with the interference of an un-scientific or possibly anti-scientific Home Secretary, we open the gates to the enemy, and must not be surprised if he takes full advantage of our weakness. We must not forget that the hysterical party have lately boasted of apparent wavering among medical men as ominous of "the beginning of the end." It is accordingly with feelings of the most profound regret that we have read, in the "Medical Press and Circular," the report of a lecture delivered before the Halifax Literary and Philosophical Society, by Mr. T. M. Dolan, F.R.C.S. (Ed.) The subject of the lecture was "The Practical Value of Recent Research on Micro-Organisms." Referring, towards the close of his address, to the Vivisection question, the lecturer is reported as having said:—

"I should like to take this opportunity of disabusing your minds about the practice, and to ask you to consider this question rationally. I shall not say a word to hurt the most ardent anti-vivisectionist. (1st) Cruelty to animals is abhorred by medical men; I need not tell you I allude to unnecessary cruelty. (2nd) Experiments on animals are only made by scientific skilled physiologists; nearly all animals experimented on are rendered insensible by curare. (3rd) All we ask for is that our tried and trusted physiologists and surgeons should be allowed, under a certificate, to conduct those experiments which they may deem necessary, without the harassing and annoying conditions which are almost prohibitory, and which are fatal to the advance of physiology. I know we have English talent in the physiological department of medicine, equal to any foreign talent, undeveloped, however, owing to the discouragement it receives. I would ask you to consider the present condition of the profession in England, and if you desire the advance of English medicine, to strengthen the hands of those who are advancing it, by your moral support.

"To torture a living animal, under the pretence of research, deserves condemnation: no language is too strong to use against such iniquity. Let the Act stand in force against such dabblers in Science; with a tenfold greater severity even than at present, let its punishment be increased, but let us discriminate. Pasteur, with all his honours thick upon him, loved, revered, with family ties, and everything that can bind him to life, goes calmly down to the foulsome Lazaretto, to try and find out the cause of

yellow fever, to stamp it out as he stamped out silk-worm disease. He goes with his life in his hand to slay disease. Colville and Payne (Englishmen) went to Astrakan to study the plague; they went forth nobly, as medical men should go, to confront the plague in its home. They exposed their lives, and I am convinced that the humblest member in my profession is so imbued with its traditions, so educated, that whether it be the cholera, the plague, or typhus he has to encounter when called in, he will be at his post, faithful unto death. It is for those unselfish men, for Brunton, Frazer, Lister, Sanderson, Greenfield, Ewart, I ask your sympathy and support; for the leaders in medicine, who are striving in this prosaic weary work-a-day world, like the knights of old, to overcome the powers that surround us, the hidden, unseen, impalpable agents of disease and death, to bring them into subjection to laws of health, to benefit you and posterity."

Let us examine this passage. There is first the promise "not to hurt the most ardent anti-vivisectionists." We may here well ask, what are their claims to such tender treatment? Is it because they have been considerate of the feelings of men of Science? Few agitators have dealt more liberally in the foulest abuse than have the leaders of this humanitarian movement. The motives, the characters, and the reputations of men, living or dead, who are known to have experimented upon live animals, have been vilified without scruple. Where the constituted authorities have refused to be swayed by clamour, our enemies have not hesitated to appeal to popular violence, as when the Physiological Institute of the University of Leipzig was stormed and ravaged by an anti-vivisectionist mob.

Or has the hysterical party merited especial consideration by the integrity and chivalrous candour they have shown? We are well aware that fanatics of all grades have somewhat confused notions of the distinction between truth and falsehood. But the anti-vivisectionists have in this respect outdone the bulk of agitators. Both in England and on the Continent they have not scrupled to represent Mr. Darwin as having expressed himself in their favour, although he distinctly says, in his letter to Prof. Holmgren, "I know that Physiology cannot possibly progress except by means of experiments upon living animals, and I feel the deepest conviction that he who retards the progress of Physiology commits a crime against mankind."

Again, at one time letters appeared in the "Standard," and we believe in other papers also, complaining that young

art students in our public galleries were worried into signing petitions for further restrictions on physiological research. These statements were from eye-witnesses. An eminent artist informed us that his daughter had been beset in like manner whilst copying a painting. Yet in the face of this direct positive evidence the secretary of one of the anti-vivisectionist societies actually wrote to deny these assertions, and trusted that the editor's "sense of fairness" would allow this contradiction to appear! We ask, How can any man truthfully contradict an account of any occurrence except he himself had been present?

Again, there appeared advertisements in different papers headed with the name of one of the anti-vivisection societies. Immediately under this heading came a list of eminent physicians and other scientific men. Any person merely glancing at the advertisement would receive the impression that these gentlemen were in some way connected with the Anti-Vivisection movement. On close examination it appeared that they were the Committee for the erection of the monument in honour of the illustrious Harvey. The purport of the advertisement was to deny, or at least to detract from, the merits of Harvey as the discoverer of the circulation of the blood. But why were the names of the Committee introduced? We have certainly no right to declare that they were inserted with the intention of deceiving the public; but we submit that men careful to avoid even the appearance of evil, and anxious not to be misunderstood, would never have placed such names in such a connection.

One more instance may serve to complete our estimate of the spirit in which this crusade against Science is being waged. According to the report in the "Daily Telegraph"—"A public meeting was held at Willis's Rooms 'to discuss the moral right and true philosophy of painfully experimenting upon animals for scientific purposes.' The president of the Society for the Abolition of Vivisection (the Rev. C. W. Grove) occupied the chair, and stated that the object of the meeting was to invite physiologists, biologists, and members of the medical profession to state their views on the subject of vivisection. It was desirable to hear both sides of the question, and to know the results of the vivisection experiments which had been made. The chairman then invited medical gentlemen to express their views. Dr. Nicholls, Dr. Walker, and other speakers condemned vivisection, and stated that they did not know of a single instance of benefit accruing to the human race as the result of vivisection experiments." So this "discussion"—this

"hearing both sides of the question"—amounted to harangues from partizans, who, if legally qualified medical practitioners, have scant right to come forward as the representatives of the profession, and still scantier claim to speak on behalf of Science.

It is further interesting to mark the motives underlying no small portion of anti-vivisectionist enthusiasm. The movement, like all agitations, ministers to the vanity of not a few individuals, and enables them to gain the ear and the favouring notice of a certain section of the public. Moreover, the competitive element, so dearly loved in modern England, is not wanting. There are several societies for the suppression of Vivisection, all in full accord as far as enmity to biological research is concerned, but jealous of each other touching the *kudos*, or the more tangible rewards to be earned. In our collection of Anti-Vivisectionist literature we find such advertisements as the following:—

"The Society has no connection with any other. The public is cautioned against persons pirating its publications, and endeavouring without any authority to obtain money."

"One of the new Societies was set on foot in the hope of supplanting the original Society and appropriating its funds by an intrigue, but the attempt was defeated. It has also pirated our publications, and we are now endeavouring to obtain legal redress. We think the public should be informed of these facts, and warned against *professional humanitarians and social parasites*, who, as is too often the case, try to obtain celebrity or to live and fatten by any humane enterprise. Like the Seigneur Manuel Ordonez, in Gil Blas, they become rich in managing the affairs of the poor." This truthful and damaging exposure of the character of the movement bears the signature of one of its most vehement champions!

Or, lastly, does Mr. Dolan wish to spare the feelings of the Anti-Vivisectionists in the hope of conciliating or disarming their hostility? It is poor policy, we would remind him, to run away from a fanatic, a lunatic, or a mad bull. He merely gathers courage from the timidity of his adversary. The present Vivisection Act was an attempt at conciliation. Its result has been merely an intensification and extension of the uproar it was intended to pacify. Are the Anti-Vivisectionists really worthy of such consideration as Mr. Dolan proposes to show them?

But to proceed: the lecturer tells us that "Cruelty to animals is abhorred by medical men: I need not tell you I allude to unnecessary cruelty." But if we define cruelty

as the wanton and needless infliction of pain and death, and especially the taking pleasure in such infliction, the very notion of "unnecessary cruelty" disappears. That which is necessary is not cruel. No one has yet had the impudence to contend that vivisectionists, whether they be medical practitioners or not, derive any gratification from their operations. To give pain for mere amusement, or for the exercise and display of skill, is something we leave to the sportsman!

Again, we read—"Experiments are only made by scientific* skilled physiologists." This is a mere truism; persons other than scientific physiologists have no inducements to make such experiments. But we can scarcely see the relevance of this contention as against the Anti-Vivisectionists. This, however, is a matter of small moment.

We pass on to Mr. Dolan's demands, which are most unsatisfactory. "All we ask for is that our tried and trusted physiologists and surgeons should be allowed, under a certificate, to conduct those experiments which they may deem necessary, without the harassing and annoying conditions which are almost prohibitory, and which are fatal to the advance of Physiology." Mr. Dolan, therefore, if faithfully reported, accepts the principle of the Vivisection Act, interpreted perhaps in a somewhat more liberal spirit than is now the case. He consents that experimentation upon animals should be restricted to "tried and trusty physiologists and surgeons," under the further limitation of a "certificate." Let us see what these admissions involve. We submit that—

1st. He places the medical profession in an exceptional and humiliating position. Both law and public opinion allow persons, often ignorant, coarse, and brutal, to perform painful operations upon animals, so long as those operations are not to promote the advance of Science. Those who take animal life for the purposes of food, those who castrate young animals, those who massacre seals in the Arctic regions to provide jackets for lady Anti-Vivisectionists, &c., do not require any certificate from the Home Office, nor does any one ask if their skill and experience are such as to minimise the suffering inflicted. Yet we venture to assert that the seal-hunt in a single season occasions a greater amount of torture to unoffending animals than Vivisectionists have inflicted for a century. Or, if a man applies to the Revenue officials for a gun- and a sporting-license, no question is

* Are there any non-scientific physiologists?

raised whether he is so efficient a marksman as to kill outright and not maim the birds he fires at. Surely to give all these classes of people full scope for action in their dealings with animals, and to single out the medical profession for inspection and "certificates" is a gross insult. Yet, according to Mr. Dolan, the perpetuation of this insult is "all we ask for"!

2nd. If certificates for experimentation are to be granted only to those who are "tried and trusted," the young, and those who have not already earned a sufficiently high reputation, will find themselves necessarily excluded. This will prove a most unfortunate restriction for the advancement of Physiology. It is universally conceded that the mind is most suggestive—most fruitful in novel ideas—in the earlier portion of life. If a biologist is to be debarred from working out and testing his ideas experimentally till he is a man of established reputation, very much important work will perforce remain undone. What would be the position of chemistry or of physics if the student must obtain a certificate before entering upon original research?

3rd. Unless we misunderstand him, Mr. Dolan completely ignores non-medical biologists. There are numbers of scientific men who in their researches into the phenomena of organic life—especially among the lower animals—find it necessary to have recourse to experiments which a sentimentalist might think fit to proclaim as painful. True such inquiries are often beyond the grasp of the present Vivisection Act, which does not include the Invertebrata in its provisions, but our fanatical party are clamorous for its extension. Chemists, also, in toxicological cases before the passing of the Act, often found it useful to administer some substance isolated from the body of a poisoned person to an animal, as a proof of its nature. Thus the most sensitive test for strychnine is a frog. But in England this test is no longer available. There is no time to wait for the various formalities, and no certainty that when all have been duly gone through the license will be granted.

Mr. Dolan's line of action, in confining his remarks to the needs of medical men, is the more strange since no small part of his lecture is devoted to an exposition of the researches of M. Pasteur. Now M. Pasteur is no surgeon, no medical practitioner. He is a chemist, formerly a pupil and assistant of Prof. Dumas, and when he first attacked the silkworm disease it was objected to him that he was no physiologist. Yet his experiments on the propagation of zymotic disease and on its counteraction have required

vivisection,—not, indeed, "cutting animals up alive," as vivisection is defined when it is desired to harrow up the public mind, but still operations which in England would lay him open to prosecution. Now, except we can secure something more than the "all" which Mr. Dolan asks for, an English Pasteur would, we think, find himself shut out.

But the Lecturer continues in a style almost worthy of the author of the "British Dog," of Miss Cobbe, or of W. Howitt:—"To torture a living animal under the pretence of research deserves condemnation; no language is too strong to use against such iniquity!" We ask Mr. Dolan whether any such cases really occur, or have occurred? Those who take a pleasure in cruelty know well that "research" is the most unsafe "pretence" they could put forward. Let them only plead "sport," or a wager, then, if the victim is a wild animal, they may either escape punishment altogether, or at most incur a fine of a few shillings. That wanton cruelty is abundant, rampant, in this country we admit, but it is not inflicted in the service of Science. Unless Mr. Dolan knows as a fact, capable of being proved to the satisfaction of a Court, that such torture under "pretence of research" does occur, he has been exceedingly ill-advised in using the language which we have quoted. He must, on reflection, feel that by so doing he has played into the hands of the enemy, who will quote his remarks as the admissions of a defender of vivisection. He goes on, however:—"Let the Act stand in force against such dabblers in Science, with a tenfold greater severity even than at present; let its punishment be increased, but let us discriminate." Here Mr. Dolan somewhat changes his point of view. In the last sentence he was dealing, as we understand, with persons who make a mere "pretence" of Science. But a "dabbler" is something different from a "pretender," and we should like to see his definition of the former term. Is it a person outside the pale of the medical profession? Or one—medical or non-medical—who has not yet acquired celebrity? Or is it some person whose skill is questionable, or whose object in experimentation is not sufficiently definite? These are points which it will be difficult to decide fairly, and which, so long as the miserable Act stands, will always be cropping up. Vivisection is troublesome, expensive, and every way unpleasant, and we cannot conceive of any person having recourse to it as an amusement, or if he sees any other way of solving the problems before him. But why even a bungling vivisector should be punished so much more severely than a cock-fighter, or than

the other classes of persons who indulge in utterly wanton inflictions of pain, it might be difficult for Mr. Dolan to show. We have no doubt that the intentions of this gentleman were excellent, but we fear that he has gravely compromised the movement which must now be made to stay the march of fanaticism. The naturalists of England are, we believe, fully prepared to stand shoulder to shoulder with the medical profession, but they cannot consent to any line of defence by which they would be abandoned.

III. PHRENOLOGY AND ANIMAL PSYCHOLOGY.

By J. W. Slater.

IT is well known that a feud exists between the naturalists and the metaphysicians, or, as they prefer to call themselves, philosophers, on the subject of animal psychology. The latter gravely decide on *à priori* principles, or indeed sometimes assume without any grounds at all that "mere brutes" cannot lay claim to some particular faculty of the mind. The naturalists, on the other hand, true to their training, allow themselves to be guided by facts. If they observe in any animal the manifestations of a certain faculty or sentiment, they at once admit its presence and do not seek to evade the issue by cunningly drawn definitions proposed for the very purpose of creating a "great gulf" between man and his "poor relations." Hence it may be suspected that metaphysical subtlety is rather a hindrance than an aid in the study of the animal mind.

Now it is somewhat singular that the phrenology of Gall, Spurzheim, and Combe, though in many respects hostile to the psychology of the metaphysical schools, entered into an alliance with them by assenting to the assumption of a distinction *toto cælo* between "man" and "brute." This view the phrenologists expressed in their arrangement of the "sentiments." They proposed one class as "common to man with the lower animals," and another as "proper to man." Under the former head they placed "self esteem, love of approbation, cautiousness, and benevolence." Under

the latter they included "veneration, firmness, conscientiousness, hope, wonder, ideality, wit, and imitation." From this classification certain corollaries would follow, *e.g.*, that all animals, man excepted, differ little mentally among themselves, and that if on the phrenological theory each faculty of the mind has its seat in some especial portion of the brain, then a whole region of the brain present in man must be absent in the anthropoid apes, which would establish a very striking difference at once recognised by the anatomist.

In the very incomplete state of our knowledge of animal psychology, it may not be useless to examine whether the sentiments assumed by phrenologists as peculiar to man are really and truly absent in the rest of the animal creation. For our purpose it will not be necessary to enquire whether these sentiments are in their nature simple and elementary, or whether any of them may result from the interaction of other primitive faculties.

We will begin at the end of the catalogue. Have the lower animals the desire to imitate the actions, manners, and gestures of other animals or of man? Affirmative instances are so common and obvious that we cannot well understand how the question can be raised by any person in possession of eye-sight. Imitation is as decidedly a leading or misleading propensity of animals as of men. It is distinctly prominent among sheep and monkeys, with the difference that a sheep only copies the actions of another sheep, whilst the so-called Quadrumana will very cleverly imitate the conduct of such human beings as they have the opportunity of observing—a characteristic not recurring with equal distinctness in any other mammalian group. I know no instance of a dog or a cat being led to eat any unusual substance through seeing its master partake thereof. I have, however, witnessed two distinct cases of imitation on the part of cats. A daughter of mine was waving a sheet of paper up and down in front of a fire to dry it, when a favourite kitten which was sitting on the hearth and intently watching, waved its right fore paw up and down in the very same manner. On another occasion a young cat sitting on a flight of steps, watched a charwoman who was cleaning the flags of the area below. To my surprise the cat began to work its fore-paw round and round, imitating the rotatory movements of the woman's arm.

When a body of hunters dash through a pasture-field where cattle are grazing, the latter have been know to join

in the chase and clear hedges and ditches in a style little in keeping with their usual sobriety of demeanour.

Wit is another of the sentiments assumed as peculiar to man. Of course, as we do not understand the language of any of the lower animals we cannot show that they indulge in humorous remarks. But it may be fairly assumed that a sense of the ridiculous, a comprehension of the nature of ridicule are causally connected with the sentiment of wit, so that where the former are present the latter cannot be altogether absent. Now certain animals are sensitive to ridicule and are greatly annoyed and offended if laughed at. That this is the case with dogs many persons know from their own observations. For the benefit of others I quote an instance narrated by Mr. G. J. Romanes, in "Nature," May 27th, 1875. A certain terrier "used to be very fond of catching flies upon the window-panes, and if ridiculed when unsuccessful was very much annoyed. On one occasion, in order to see what he would do, I purposely laughed immoderately every time he failed. It so happened that he did so several times in succession—partly, I believe, in consequence of my laughing—and evidently he became so much distressed that he positively pretended to catch the fly."

It may here, of course, be contended that dogs by constant companionship with man, may have acquired feelings or sentiments not natural to the lower animals. To this objection I give my usual reply; if circumstances can create in a dog or other animal some sentiment entirely new, then the presence or absence of any mental feature is utterly inadequate to establish a distinction of kind between man and beast. But the same feeling is shown by monkeys, born in a wild condition and existing as man's captives rather than as his companions. This fact throws a strong light into the recesses of brute nature. A baboon to be annoyed at ridicule must understand human gestures, play of features, and tones of voice; and, more than this, he must be himself capable of finding the conduct, appearance, &c., of other beings ridiculous. How else should he comprehend the very nature of derision?

I turn next to ideality—that fondness for beauty which in man comes to light in phases so various; from the craving of the savage for beads and feathers to the love for sublime scenery, for painting, sculpture, and poetry among the leading races. Do brutes appreciate ornament? Beyond all doubt; horses and elephants value their trappings and grow sulky if these are removed.

The phrenologists have, I believe, attempted to refer these

facts to "self-esteem" or to "love of approbation." But these sentiments in such a case can only be brought into play through a love of ornament. Between the animals in question and the human being who glories in a compressed skull, in a tattooed skin, in a porcupine's quill thrust through the nostrils, in compressed feet, high-heeled boots, a powdered face or head, or a wasp-waist, the interval is not very wide.

A love of ornament may be constructively shown in wild animals. It is well known that many birds at the pairing season assume, wholly or in part, a more splendid attire, described in ornithological works as the "nuptial plumage." Does not, as many of the most eminent naturalists now hold, this brilliant coat of feathers serve the purpose of attracting the opposite sex? If so the effect could not be answered in the entire absence of a passion for ornament and of a sense of beauty. The Australian bower-bird gives plain proof of this sentiment. In the construction of his gallery he makes use of the most showy objects he can find, and takes great pains in their selection and arrangement. Neither the nature of the materials nor the very existence of the gallery seems to subserve any mere utilitarian object. Some may, perhaps, say that "instinct" prompts the bird to collect and arrange all these baubles. But if this explanation can be accepted it will equally apply to the savage in his fondness for the very same articles. Perhaps the fact—now thoroughly demonstrated—that monkeys can recognise the likeness, coloured or plain, of a member of their own or of some closely-allied species, may here be taken into account.

Next follows in our list the sentiment of "wonder." In virtue of this feeling man, when he recognises anything outside the ordinary sphere of his experience, is more or less profoundly affected. Whether his interest in such phenomenon takes the form of terror or of pleasure—at least attraction—depends on a variety of incidental circumstances. But brutes also recognise an order of nature and are startled by its apparent violation. Of this I once met with a striking instance. A dyer had a favourite poodle upon which he exercised his art so successfully as to change its fleece from white to a full shade of magenta. I one day observed this animal following its master along a street, when a terrier ran hastily up to accost the stranger. On coming closer and perceiving the un-dog-like hue of the poodle he stopped short, gazed for a moment as if bewildered and then fled. Fear—in the sense of physical

apprehension—could hardly be the cause of his flight, as the poodle wagged his tail and gave every sign of friendliness.

Mr. Romanes—if I remember rightly, though I am not at the moment able to lay my hands upon the exact passage—describes the conduct of an otherwise pugnacious terrier on seeing a monkey dressed in human costume. He stood at first in astonishment, but, recovering, was about to make an attack on the strange being, when the monkey raised its hat and gracefully saluted him. This was too much for the terrier's nerves; he slunk away and was seen no more till the monkey had been removed.

Mr. Herbert Spencer ("Principles of Sociology") gives some cases of wonder or mysterious alarm displayed by dogs at occurrences transcending the limits of their experience. "One of these cases consisted in a large dog which, while playing with a stick, accidentally thrust one end of it against his palate. Giving a yelp he dropped the stick, rushed to a distance from it, and betrayed a consternation which was particularly laughable in so ferocious looking a creature. Only after cautious approaches and much hesitation was he induced again to take hold of the stick." Mr. G. J. Romanes, commenting upon this case, gives further instances of awe, horror, or sense of the mysterious in dogs, which have come under his own observation. Speaking of a certain terrier he writes:—"Like many other dogs he used to play with dry bones by tossing them into the air, throwing them to a distance and generally giving them the appearance of animation in order to give himself the ideal pleasure of worrying them. On one occasion I tied a long and fine thread to a dry bone and gave him the latter to play with. After he had tossed it about for a short time I took the opportunity when it had fallen a little distance from him and while he was following it up, of gently drawing it away from him by means of the thread. Instantly his whole demeanour changed. The bone which he had previously pretended to be alive now began to look as if it were really alive. He first approached it with nervous caution, but as the slow receding motion continued and he became certain that the movement could not be accounted for by any residuum of the force which he had himself communicated, his astonishment developed into dread as he ran to conceal himself." The dog, Mr, Romanes adds, was under ordinary circumstances by no means timid. He describes another experiment performed with the same animal:—"Taking him into a carpeted room I blew a soap-

bubble, and by means of a fitful draught let it intermittently glide along the floor. He became at once intensely interested, but seemed unable to decide whether or no the filmy object was alive. At first he was very cautious and followed it only at a distance, but as I encouraged him to examine the bubble more closely, he approached it evidently with much misgiving. After a time, during which I always kept at least one bubble on the carpet, he began to gain more courage, and became bold enough slowly to approach one of the bubbles and nervously to touch it with his paw. The bubble, of course, immediately vanished, and I never saw astonishment more strongly depicted. On then blowing another bubble I could not persuade him to approach it for a good while. At last he came and nervously extended his paw with the same result as before. But after this second trial nothing would induce him again to approach a bubble, and on pressing him he ran out of the room, which no coaxing would persuade him to re-enter."

Mr. C. G. O'Brien, of Cahirmoyle, County Limerick, writing in the same paper, gives also a striking instance of the sense of the mysterious in a dog. He says:—"Our terrier, a very queer character and a great warrior, is abjectly superstitious. He will not come near a toy cow that lows and turns its head, but watches it at a distance with nose outstretched. A vibrating finger glass terrifies him; indeed he has so many superstitions that we often make him very miserable by working on his fears."

These instances abundantly prove that animals, like man, possess a sense of the marvellous. Though somewhat a departure from my subject I venture to point out that this is at the same time the surest proof of their possession of reason; unless they had some crude conception of an order of nature—some power of distinguishing between the probable and the improbable—nothing could excite their wonder.

Hope, the next emotion in the list, is easily dispatched. Unless dogs or cats hoped to receive scraps why do they continue begging? Unless birds hoped to find to-morrow crumbs where some were thrown to-day, why should we find them assembled on a winter's morning around the breakfast-room window? In so doing they give at the same time inarticulate expression to their recognition of the law of continuity.

I come to conscientiousness. The lower animals have the sentiment of right and wrong, of claims and duties. Rooks, for instance, have some rudiments of criminal law.

Inveterate thieves are sometimes severely beaten, banished from the rookery, or even killed outright. These punishments are inflicted at a general assembly of the rookery. Now law pre-supposes some notion of right and wrong, and could, therefore, never arise among beings incapable of making this distinction.

The following incident, which gives us a most valuable insight into the moral life of brutes, is recorded by Arago in his official *éloge* of Ampère. There is probably no scientific man in the world who would hesitate at recognising the latter as a trustworthy observer, utterly incapable of exaggerating or distorting any phenomena which he had witnessed. Travelling in the South of France—the exact locality is little to the purpose—Ampère had occasion to make a short stay at a road-side inn where a roasting-jack moved by animal power was in use. Two dogs performed the duty of turning the machine, working, as it appears, alternately. The dog whose proper turn it was not happening to be in the way, the other was caught and put in the wheel. He flatly refused, however, to work, and neither coaxing, threats, nor chastisement produced any effect. After some delay the missing dog was found and set to the task. After he had nearly completed the job, Ampère, who bad been greatly struck with the whole affair, proposed that the second dog should be released and the first tried again. This was accordingly done, and the animal so lately recusant now offered no opposition, and made the wheel revolve with a hearty good will till the roast was finished. This experiment showed that the dog's former reluctance was due not to idleness or ill-temper but to principle. He evidently considered that the work was to be fairly shared between himself and his companion, turn and turn about. This incident proves that brutes are able to understand somewhat complicated relations. Had the dog been willing to work whenever ordered, his compliance would have been simply attributable to docility, or fear, or to habit. We find him, on the contrary, obedient up to a certain point, but no further. What is this but the idea of duty and of right? He felt bound to work alternately with his colleague, but when expected to take two shifts in succession he knew that more than his legitimate duty was demanded, and that his rights were consequently invaded. This case alone is quite sufficient to prove that the sentiment of conscientiousness crosses the boundary-line between man and the brutes. It shows that the "actions of animals *are* accompanied by acts of conscious will directed towards the fulfilment of duty.

As to veneration its presence in the animal world has been established by hundreds of observations, summed up in the well-known aphorism "man is the God of the dog."

There remains merely firmness, which carried often to the extent of obstinacy is as plain among animals as is imitation.

We find, therefore, that all the "sentiments peculiar to man" of the Phrenologists are common to him and to some, at least, of the lower animals. How such an erroneous classification of the mental faculties came to be adopted is difficult to understand. Probably Gall, Spurzheim, and Combe were not close observers of animal life and followed the "learned ignorance" of their day in denying to "brutes" all the higher sentiments. Should the science of Phrenology ever be re-constructed in a form capable of harmonising with the results of modern research, this point must not be overlooked.

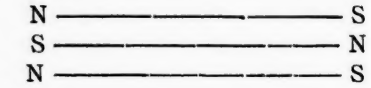
IV. THE PRINCIPLES OF MAGNETISM.

By Charles Morris.

AMONG the varied phenomena of magnetism, there is one of more significance than has usually been ascribed to it, and which, when considered in all its bearings, would seem to lead us to a fuller knowledge than we now possess of the intimate relations of magnetic particles. This is the fact of the astatic combination into which every two contiguous magnets seek to enter. As a necessary consequence of magnetic polarity, with its mutual repulsion of like and attraction of unlike poles, any two magnets laid side by side will repel each other if their polar axes be in the same direction, and attract if their axes be in opposite directions. This is one of the fundamental principles of magnetic energy, but it is one which has never been worked out to its ultimate significance.

If, instead of two, there be several magnets thus laid side by side, the effort made by each to reverse the polar axes of

all the others must cause special relations of axial direction. If free to move in response to this influence, every magnet situated between two others would arrange its poles oppositely, and the combination would be some such as this:



If a number of magnets were arranged so as to form the surface of a hollow cylinder, and, as above, free to shift their axial directions, the relation here given must be assumed around the whole surface, and the poles of the magnets be successively opposite in direction. Of course, if in the above diagram there were five magnets instead of three, the influence of the second would tend to prevent the reversal of the fourth, but the double influence of the third and fifth, and the increased vigour resulting from their proximity, must cause such a polar reversal to take place. But when the magnets become numerous and less regularly conjoined, the above simple arrangement cannot arise. Each magnet would be exposed to several varied influences, and must assume a position in accordance with the preponderant energy of these influences. Thus, instead of yielding to the attractive vigour of the magnet nearest it, the vigour of the mass of more distant magnets must also be felt, and the final position assumed by the magnet arise, not from the isolated influence of any single magnet, but from the resultant of the influences of the whole mass of magnets. It would be a case somewhat resembling that of general attraction in which the sum of the influences between particles constitutes a general central influence. But in the case of a mass of magnets the presence of both attractive and repulsive relations must considerably complicate the result, and the axial direction of each magnet be governed both by attractive and by repulsive influences.

In the case already assumed of a hollow cylinder composed of magnets, each of which is in astatic relation with its two neighbours, if we should now place in the core of the cylinder another magnet equal in vigour to all those forming its surface, new relations would necessarily ensue. The magnets in the surface must enter into astatic relations with the magnet in the centre, their mutual repulsions being overbalanced by the preponderant central attraction. Or if, instead of a single magnet, a mass of small magnets be placed in the core of the cylinder, some similar result

would probably arise. The tendency of every two contiguous magnets would still be to enter into astatic union, but this would be overbalanced by the sum of distant forces. Most probably the final relation would be that given above; the central mass of magnets would control the axial positions of the surface magnets, and these exert a like influence upon the centre, while the isolated local influences would be overcome. Thus repulsive relations might exist between contiguous magnets, as a result of their attractive relations with masses of more remote magnets.

For such relations to be assumed, of course the separate magnets must be free to revolve on their equatorial planes. But even where they have not this freedom of movement the vigour of this influence often shows itself by a reversal of the magnetism of some of the magnets. Such a reversal is stubbornly resisted, and the energy producing it must be considerable. We have instances of this result in the experiments of Coulomb and of Nobili. The former found on examining bundles of magnets that the exterior plates were the strongest, and that some in the interior had their magnetism reversed. Nobili experimented with bundles of fifty small needles which had been powerfully magnetised. On opening the bundles after a short time, all the magnetism continued in the same direction. But on opening again two hours afterwards, several of the needles were found to have reversed their magnetism. In another experiment, part of the needles lost their magnetism. In cases like this a general reversal could not well take place, for every magnet so reversed would exercise a counter force, and diminish the reversing influence upon the remaining magnets. And any vigour of retention of the original magnetism would also oppose the reversing agency.

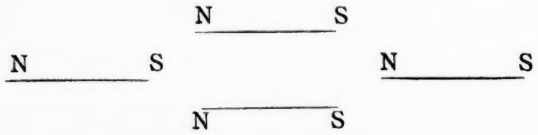
It may be supposed that the other relation of magnets alluded to—that of freedom to rotate and so change the direction of their axes—could not be produced in practice. Yet the practical existence of this relation may be found in every instance of magnetic energy. For it is well known that magnetism is not an energy belonging to masses as wholes, but that it belongs to their molecules or separate particles, and that what we call a magnet is really a compound mass of minute molecular magnets. In such an aggregate of minute magnets the conditions are different from those applying to masses of larger magnets. In the case of molecules there is no physical resistance to a change of the axial direction. They have no need to reverse their magnetism in response to the surrounding forces, for they

are free to readily reverse their positions. Consequently it is very probable that every molecule in such a mass feels the magnetic energy of every other molecule, and freely shifts its axis into the direction required by the superior attraction. The case is like that which would exist between larger magnets if so arranged as to have liberty to revolve, and so placed that the action of their mutual energies could not be counteracted by terrestrial magnetism.

But the side-by-side relation of magnets so far examined needs to be considered in connection with another relation ere we can comprehend the true magnetic condition. This is the end-to-end relation. If two magnets which, laid side by side, would repel—be shifted without a change of axial direction until they become end to end—they will attract. Or if in the first instance they attracted, in the second they would repel. And this relation of magnets must influence and vary the relations we have hitherto considered. Thus the tendency of two magnets to arrange themselves astatically, thus:—

N ——————— S
S ——————— N

would be counteracted in the following arrangement.



The two end magnets would attract the upper one of the astatic pair, and repel or cause a reversal of poles in the lower one. In the new relation assumed the paired magnets would repel each other, but their repulsion would be overcome by the attraction of the end magnets.

Let us again consider a cylindrical mass of magnets, made up of unit magnets arranged thus end to end as well as side by side. The tendency of the magnets in the core of the cylinder to assume an astatic relation with those in the surface, and for this agency to overcome the local influences of magnet on magnet, must be aided by the above arrangement. In any group of surface magnets like that in the last diagram, the influence of the central magnets of the cylinder would be aided by the energies of the two end magnets of the group, and the astatic tendency of any two contiguous magnets would be thus doubly resisted. But at

the same time, the astatic tendency between the surface and the central groups of magnets would be assisted. Consequently, in any such magnetic system, the special relations which tend to establish themselves between contiguous magnets must be overcome by general relations between the various sections of the group, most probably resulting in the above condition of astatic relations between the superficial and the central sections of the system. As already said, such a relation could only partially display itself in any group of mass magnets, on account of their resistance to reversal of magnetism. But it could readily appear in groups of molecular magnets, from their ability to shift their axial directions. We are, therefore, not without warrant for the conclusion that in every magnet this relation of its molecules exists, and that the magnetism of the central portions of the magnet is opposite in direction to that of its surface portions.

But still another conclusion must arise from these premises. The conditions here assumed could only exist in the equatorial plane of the magnet. If we take the somewhat parallel case of a globular mass, every particle of which is drawn by attraction towards the centre, we know that this is not because of a force of attraction existing in that centre, but because the attractions of all the remaining particles have their greatest sum in the line passing through the centre. But in the case of a magnetic mass the forces, though in one sense general, are in another sense special. The magnetic particle on the equator of the magnetic mass would be equally influenced by the northern and southern sections, and would thus obey on equally balanced axial influence. But a magnetic particle out of the equator, and nearer one pole than the other, must feel a preponderating influence from the greater mass in the direction of the more distant pole. It would not arrange itself, therefore, in true astatic parallelism with the axial particles; but, if it be nearer the North Pole, it must be affected most strongly by the greater mass towards the South Pole, and thus be thrown out of its reverse parallelism with the axis. As it comes nearer and nearer to the North Pole this reversing influence from the South Polar region must grow more effective, until, when the North Pole is reached, the magnetic particle must become fully reversed, and in direct parallelism with the axis. All this may seem a far reaching deduction from the astatic tendency of any two magnets. But it can be shown that it is in exact agreement with the ordinary phenomena of magnetism.

Although we cannot see the molecules of ordinary magnets, and thus perceive their movements in response to the general magnetic energy, yet we possess one instance of a magnet whose magnetic molecules are plainly visible, and in which we can trace the operation of the principle just laid down. This great magnet is the earth. Its molecules are its crystals, or the other definite units of matter of which it is composed. In the magnetic needle we have a strongly magnetised terrestrial molecule, by whose aid we can test the conditions of the earth's magnetism.

In the ordinary consideration of the earth's magnetic phenomena there is, what seems to the writer, a misconception of the true relations between the earth and the magnetic needle. They are treated as if they were two separate magnets, and their action upon each other viewed in the same light as we view the mutual action of two ordinary magnets. Yet the fact is that the needle forms part of the earth's solid surface, and its magnetism is part of the earth's magnetism. We speak of the north and south poles of the earth with the tacit idea that all the magnetism between these poles lies in the same direction, and that every one of the separate magnetic masses, whose total sum of force composes the earth's magnetic energy, has its poles turned in conformity with the direction of these north and south poles. Yet the behaviour of the magnetic needle should give us a different idea. At the equator it turns its poles opposite to those of the earth. And this is not because it stands above the immediate surface, for if laid upon, or buried beneath and incorporated with this surface, it would not change its axial direction. Only when taken from the equator towards the poles does it gradually change this direction, its axis becoming completely reversed only when the poles are reached.

In all this we have a significant indication of the true conditions of terrestrial magnetism. Although all matter is magnetic, only a small portion of the earth's material is strongly so, though the weak magnetism of the remaining material may slightly add to the total sum of magnetic energy. These strongly magnetic masses are disposed at random throughout the earth's crust, and it is their mutual action which constitutes the main sum of the earth's magnetic energy. In the action upon the needle we cannot assume that some hypothetical concentration of energy at the north and south poles controls its movements, but must rather consider it as principally affected by the vigour of the contiguous magnetic masses, and as assuming a position in

accordance therewith. The needle must tend to enter into astatic relations with these masses. But these masses are numerous, and all tend to assume astatic relations with each other. Yet they cannot all assume this relation. The mutual action of any two of them is hindered by the action of the many more distant masses. The result is that local relations of position cannot be assumed, but that general relations must arise, the magnetic masses in one section of the earth becoming astatic with those in another section. The direction assumed by the needle, for instance, is not astatic with that of the superficial magnetic masses of the earth, for they undoubtedly agree with it in direction. It must therefore be in astatic relation with more deep-lying masses. But the needle points in the same direction at every point in the equatorial surface. It follows that the astatic tendency between the superficial magnetic masses is at every point opposed by a constant energy which can only come from the deep-lying figures of the earth. Thus we may view our globe in its equatorial plane, and for some distance to the north and south, as constituting a cylindrical mass, the magnetism of whose superficial regions is opposite in direction to that of its central regions, while the north and south pointing of surface magnets must arise from their astatic relations of attraction with south and north pointing magnetic masses in the earth's interior. The relation, in short, is that to which the consideration of the mutual action of minute magnets led us. Instead of each two separate magnetic masses entering into individual local relations, the whole sum of magnetic masses enters into general relations, and the earth possesses an axial magnetism opposite in direction to its surface magnetism.

The axial magnetism occupies the whole extent of the earth's axis, and displays itself at the poles, the axis being everywhere equally affected by the surrounding exterior surface. (This, of course, requires that the earth should be throughout equally magnetic, and the irregularity of its actual magnetism must produce a certain degree of inharmony in the result.) But on the surface that action obeys another rule. If we take the magnetic needle northward from the equator, it at once feels a preponderant influence from the greater mass of magnetic matter to the south. It no longer continues parallel with the earth's axis, but "dips," its astatic relations with the axis being disturbed. The axial influence upon it may continue the same, but the surface influence is no longer balanced. At the equator the equal "end-to-end" pull of the superficial masses to the

north and south aided the reversing influence of the axial magnetism. But at a distance from the equator these surface influences are no longer equal. If it move north the sum of the whole surface influences to the south overbalances those to the north, the axial influence is partly overcome, and the magnet dips its north pole towards the earth's axis. Going still farther north this influence increases, and the reversing movement strengthens. At a certain distance north the reversion is half completed, and the magnet stands perpendicular to the earth's axis. Continuing the northward movement the magnet continues to turn, until, when the extremity of the earth's magnetic axis is reached, a complete reversion is produced, and the axis of the magnet stands opposite to its position at the equator. It has now become a portion of the earth's axis, and all the surface influence is combined to hold it in this position.

The dipping movements of the magnet here considered undoubtedly indicate similar conditions in the earth's magnetism as a whole. At the equator the earth's superficial magnetism is opposite in direction to its axial. Going north from the equator a reversing influence shows itself, the direction of the magnetism slowly revolves, at a certain distance north it becomes vertical to the axis, still farther north the angle becomes greater, and at the magnetic north the reversion is completed, and the axial magnetic direction assumed. Going south the same series of changes occurs, though in the opposite direction. The magnetic needle, therefore, may be looked upon as a testing magnet, through whose movements we may become aware of the true magnetic conditions of the earth. The complete revolution on its equatorial plane, which it makes on passing from the north to the south pole, is significant of a like complete revolution in the earth's superficial magnetism, while its assumption of the same direction at the two poles is indicative of a fixed direction of magnetism throughout the whole extent of the axis.

Were the earth's magnetism very powerful all local action would be lost in the general energy, and we could obtain no knowledge of magnetic phenomena. As it is, however, the mutual influences of two powerful magnets are stronger than that of the earth upon either, and they produce an astatic effect upon each other. But on being separated to a certain distance the terrestrial vigour overpowers the local, and they place themselves—if free to move—in conformity with this general vigour. Doubtless similar astatic influences must arise interiorly when any two magnetic masses are conti-

nuous, and the general action of the earth's magnetism must be weakened by such local reversing action.

If now, instead of taking our testing magnet over the earth's surface, we take it inward towards the axis, other phenomena than those above considered must arise. If this be done in the plane of the equator, the necessary reversal of magnetism as the axis is approached cannot take place by means of dip, for the equality of northern and southern influences produces an equal tendency to dip in the two opposite directions. It can only take place, then, through a weakening and gradual disappearance of the magnetic energy, and its reappearance in the opposite direction as the axis is neared, until finally a full opposite energy is gained at the axis. Thus between the astatic regions of axis and surface there must be an intermediate region of weaker, and a zone of no magnetism, produced by these opposing influences.

But north and south from the equator the conditions are necessarily different. If the testing magnet be carried inwards at a point between the equator and the north pole, "dip" phenomena must appear. If it be carried southward from the north pole over the surface a dip reverse to that displayed when it is carried from equator to pole will occur. And if, instead of continuing this movement to the equator, the magnet be carried beneath the surface at any point sufficiently southward, its reverse dip must continue and grow more declared, from the increased vigour of the superficial influence. At a certain depth beneath the surface, then, it would assume the direction which it possesses at the equator, that of reverse parallelism with the axis. Going still deeper the reversing influence must continue, and become complete at the axis. Thus the effect upon the magnet, of thus taking it inward to the axis, would probably be the same as if it had been taken on over the surface to the south pole, a complete rotation of the direction of its axis taking place in either case. Of course it cannot be positively declared what would occur in the case of such a movement beneath the surface, but the action of the astatic influence must certainly tend to produce such a continued rotation.

(To be continued.)

ANALYSES OF BOOKS.

American Nervousness: its Causes and Consequences. A Supplement to "Nervous Exhaustion." By G. M. Beard, A.M., M.D. New York: G. P. Putnam's Sons.

We find here, re-arranged and harmonised, ideas with which the scientific world has been already more or less familiarised, by the author's memoirs in a variety of American journals, and by critiques which have appeared on this side of the Atlantic.*

Dr. Beard considers "nervousness," or, more strictly speaking, nervelessness or lack of nerve force, as a characteristic phenomenon of the present age, and most fully developed in the northern and eastern portion of the United States. There he proposes to study it by that perfectly legitimate scientific artifice which bids us investigate every phenomenon by preference where it occurs in the highest perfection. But here already there is matter of discussion. Dr. Beard uses such language as—"This condition is especially frequent and severe in the northern and eastern portion of the United States;"—"the greater prevalence of nervousness in America;"—A new crop of diseases has sprung up in America of which Great Britain until lately knew nothing or but little;"—"nervousness and the functional diseases derived from it are most frequent in America," &c. In these and in many other passages he states more or less explicitly that the condition of defective nerve-power is more common in certain parts of the American Union than elsewhere. But Mr. E. C. Towne, writing in the "Popular Science Monthly," tells us that "the worn, thin, nervous type is fully as common in England as in America, and often more extreme here than in America;"—"the John Bull type is an exceptional one in England."—"The impression that the English are phlegmatic is a false inference."—"Temper, in fact, and nerves are generally very much worse in England than in America." These utterances seem little in accord with the opinions of Dr. Beard. Yet both writers know the two countries and their inhabitants from personal observation. Still it must be noted that Dr. Beard includes among the signs of the nervous diathesis "susceptibility to stimulants and narcotics and various drugs, and consequent necessity of temperance." Mr. Towne thinks that there would be little difference between the two peoples "if England had had American abstinence for the past fifty years." He states that the characteristics of the John Bull

* *See* Journal of Science, 1877, p. 121, and 1875, p. 130.

type are "due to English eating and drinking." Again, he speaks of the "puff and colour given to the face by drink." Thus temperance is not in England, as in America, necessitated by climate or other circumstances, and it is possible that Dr. Beard's "nervousness" and Mr. Towne's "nervousness" are not exactly one and the same thing, the latter writer meaning rather exaggerated nervous activity.

Our author enumerates, among the reasons for the greater prevalence of nervousness in America, "dryness of the air and extremes of heat and cold." Mr. Towne declares that "the drying irritating corrosive effect of the east wind in the streets of London is very much worse than anything known in America;" . . . "the irritating effects of the English climate are more than twice as bad as those of the American." . . . "England has ten days of irritating rawness, dryness of cold wind, and poison of dust, to one that America has."

Again, Mr. Towne says, "As for heat, the mistake is no less complete. England suffers more from 78° than America from 98°. There is never a chance to change to summer dress with any security, and heat, when it does come, has to be undergone without preparation. It commonly, moreover, comes with excess of moisture, and has an effect more dangerous than 20 degrees more of American heat." Now we have no personal experience of American weather, and have not the faintest disposition to do battle for the English climate, and especially for our north-east winds. But for all this we must call certain of Mr. Towne's positions in question. In America, during the summer season, sun-stroke is a very tangible evil. In England the heat is never so high as to be dangerous to a man of fair constitution and temperate habits. If in this country we "undergo heat without preparation," the reason is not so much the uncertainty of our climate as our stupid subservience to the dictates of fashion. Half a century ago nankeen, fancy drills, and white jean were common materials for male attire, but these have all been condemned by Mrs. Grundy. Not long ago we read an article by an American writer on the snow-storm of January 18th last. He was particularly struck with the manner in which we were socially paralysed by what in his country would be deemed a mere routine occurrence. Dr. Beard remarks that an English January is merely like an American March "stripped of some of its most objectionable features and on its best behaviour." Our east winds he considered tame and mild as compared with that compound of snow-dust and Cayenne pepper with which New England is scourged. So that on the question of climate experts differ.

The most remarkable point in Dr. Beard's work is that he traces nervousness, with its morbid developments, to the modern type of civilisation, especially as manifested in America, as the five distinguishing features of which he enumerates steam-power,

the periodical press, the telegraph, the sciences, and the mental activity of women. Elsewhere he goes more into detail, and adds as causes of nervelessness the political machinery of free countries, the religious excitements which are the sequels of Protestanism, the activity of philanthropy made necessary by the increase of poverty and certain forms of disease, the complexity of modern education, the over-strained division of labour in manufactures, the necessity of punctuality, the effect of noise on the nerves, the increase in the amount of business done in modern times, and the repression of emotion. We notice in particular the following remark:—"A modern philosopher of the most liberal* school states that he hates to hear one laugh aloud, regarding the habit, as he declares, a survival of barbarism!"

We may now comment briefly upon some of these factors in the production of nervousness. We may first notice how completely the introduction of certain mechanical facilities and appliances has disappointed the hopes with which it was at first greeted. When the steam-engine made its appearance as a substitute for manual labour we were told by the poets and the philanthropists that mankind would have more leisure,—there is a world-wide difference between leisure and idleness,—and consequently more scope for thought, for self-cultivation, and for all those innumerable phases of work which, however important to the community at large, have no immediate market-value. Instead, however, of doing as we might have done, getting through our former quota of work in a fraction of the time previously required, and devoting ourselves to "plain living and high thinking," we have elected still to devote our whole time to business, and to accumulate luxuries which we have neither time nor energies to enjoy.

The distressing effects of noises on the nerves are also noticed, though the author omits the most irritating of all, *i.e.*, the barking of dogs, church bells, and street music.

On political and theological excitement Dr. Beard writes:—"The experiment attempted on this continent of making every man, every child, and every woman an expert in politics and theology, is one of the costliest of experiments with living human beings, and has been drawing on our surplus energies with cruel extravagance for the last hundred years." It is a remarkable fact that insanity is rarer under an absolute government than in a constitutional or a republican country,—rarer, too, in Catholic or Islamite than in Protestant nations. Spain furnishes few cases for the asylum. Madness is far less frequently met with in devout and royalist Bretagne than in the free-thinking departments around Paris and the other great towns of France. It becomes a serious question whether the predominant and growing attention paid to politics is not a grave mistake, and whether it

* Might not a definition of the term "liberal" be useful in these days?

should not be in every possible way discouraged by all who look beyond and above the region of party turmoils.

The following few lines contain a terrible admission :—" Not How shall we live ? but Can we live at all ? is the problem that almost every American is all his life compelled to face." Is this the result of all our boasted " progress " ?

In connection with climate Dr. Beard mentions that Spiritualism in the Southern States is unknown, at least in comparison with the North and the East. He seems here to forget that it finds numerous and very zealous cultivators still farther South,—to wit, in Mexico and in the Spanish republics of South America. We are also puzzled with his statements concerning the use of narcotics. We find it maintained that increasing nervousness renders a people less able to bear the use of tobacco and opium. Yet according to statistical evidence the consumption of tobacco, both for smoking and chewing, is greater than in England. With us, too, smoking is, or has been till within the last few years, decidedly on the increase, *pari passu*, with nervousness. We are somewhat surprised to learn that " through all the Northern States the brain-working classes find coffee more poisonous than whisky or tobacco, and thousands are made wakeful by even a gentle cup of tea."

As a means of combatting the decrease of nerve force he recommends a re-modelling of our educational systems. He enjoins rest, and, like every other man who has escaped infatuation, he condemns cram and its necessary cause—competitive examination. He advises the student in every science to start with the examination of things. He does not consider the acquisition of ancient languages a necessary part of general culture. Under the same head might rank history, which is little better than fossil scandal. Its study was formerly recommended on the plea that it made men patriotic ; but our professors of history have quite outgrown patriotism. The load of lumber which the American student is required to bear upon his shoulders is even greater than in this country. The following passage is truly painful :—" The newspapers and novels that he (the American youth) is and must be prepared to criticise, the sermons and lectures which he is compelled to listen to and analyse ! " Such tasks may be very well for the man of words, but why demand them from those whose future career is to lie among things ?

The author's teachings on the comparative longevity of brain-workers cannot be accepted in their entirety. The boundary line between the two classes cannot be drawn with the sharpness which he attempts. " Nearly all the money of the world," he says, " is in the hands of brain-workers." Yet that class includes multitudes of the poorest of all. Many other of the distinctions which he makes seem to us too arbitrary. But this is a subject into which we have at present no room to enter, though we may return to it on some future occasion.

A German-English Dictionary of Words and Terms used in Medicine and its Cognate Sciences. By FANCOURT BARNES, M.D. London: H. K. Lewis.

IN the present day, when the technical phraseology of almost every Science and of such Arts as are most immediately connected with them is growing with such alarming speed, a Dictionary such as the present is much needed. The scientific language of Germany is in a peculiar position; we find in it words of Greek or Latin origin similar to those so wantonly multiplied by English bilogical writers; we meet with others purely Teutonic, and capable of being "understanded of the people,"—a feature which we should do well to imitate,—and with others which are compounds, or rather mixtures, of these two incongruous elements. Hence even a very fair German scholar may often find himself bewildered, especially if he has the misfortune to fall in with a book printed in the reformed—or rather *de*formed—orthography which has been lately brought into fashion in Germany. A man may be very well acquainted with "Theerfarben," and yet wonder what can possibly be meant by "Terfarben."

Dr. Barnes undertakes to give "most of the technical words used in Botany, Chemistry, and Zoology, as well as those used in Anatomy, Physiology, Medicine, Midwifery, and Surgery." He has not been equally successful in all these departments. The chemical terms introduced are neither very numerous nor strictly scientific, being in many cases those used by pharmacists, miners, &c. They are not in all cases correct; thus we find "Wismuth salmiak blumen, nitrate of bismuth," a compound in which there is certainly no "salmiak," *i.e.*, ammonium chloride. Further, we have "Braunstein essig saures" rendered "acetate of antimony." "Braunstein" is the trivial German name for the native peroxide of manganese. "Kupferkalk" is a very antiquated name for copper oxide, and "Zinnsalz" means not acetate of tin, but hydrated stannous chloride.

Zoology has also fared but scantily. We find "Aas käfer" rendered "dung-beetle" instead of carrion beetle, *e.g.*, *Silpha*, or *Necrophorus*. "Bock-kaefer" is a name given not to the stag-beetle (which is known in German as Hirsch-kaefer), but to the larger Longicornes, such as *Cerambyx heros* and *Prionus coriarius*.

In the botanical department we find "Pappel" (poplar) rendered by the Latin word "Malva." "Fingerhut"—the German name for foxglove, is said to be an adjective, and is rendered "digital."

We perceive that some words are translated into English, and others without any apparent reason into Latin: thus "Gemeiner Hafer"—which means oats—is rendered *Avena sativa*; "Flieder"

—lilac—is given as *Sambucus niger*, whilst "Hollunder" is translated "elder." "Holztheer"—wood-tar—figures as "*pix-liquida*"; "Meer-grün"—sea-green—is interpreted "*glaucus*"; "Petersilie"—parsley—is *Apium petroselinum*. "Thymian" is correctly rendered "thyme," whilst "Thymian kraut," which means exactly the same thing, has become "*herba thymi vulgaris*." We submit that one of two courses should have been adopted; either all the German colloquial names of plants should have been rendered by their trivial English equivalents or by their strictly botanical names. The author has adopted, however, sometimes one of these courses and sometimes the other.

In human anatomy and physiology, in the names of diseases and their systems of surgical appliances and operations, Dr. Barnes is much more at home, and these departments are accordingly very complete.

In case of a future edition we feel bound to call attention to the numerous typographical errors, such as cyaninde of potassium for cyanide, Eisen-oxhydul for oxydul, Mannerschew for Maennerscheu, Urkem for Urkeim, Anerhahn for Auerhahn, urogallas for urogallus, &c.

We are well aware that a work such as that before us requires an immense amount of labour and care if it is to be accurate, and we are far from being actuated by any desire to find fault. On the contrary, we consider that Dr. Barnes has deserved well of the scientific public, and we trust that he may soon see his way to the reproduction of the work in an amended form. In such an event we beg to suggest that space might be gained by omitting such words as are the same in both languages. The groups of compounds formed from some common word might also be greatly cut down without inconvenience. As an instance we need only refer to such heads as Wunde and Wurm.

The Analysis and Adulteration of Foods. By JAMES BELL, Principal of the Somerset House Laboratory. Part I. Tea, Coffee, Cocoa, Sugar, &c. Published for the Committee of Council on Education, by Chapman and Hall, Limited.

THIS work, as we learn from the Preface, is to serve as an explanation of the specimens of adulterated foods to be seen at the Bethnal Green Museum, and at the same time as an addition to our knowledge of the composition of various articles of food, and of the methods employed in their analysis. The analyses, we are further told, are almost entirely of recent date,—an obvious truth, since the chemists of the Somerset House Laboratory were, we believe, exclusively engaged with the examination of

excisable articles till—on the motion of Sir H. Peek and Mr. Pell—they were constituted by the late administration an ultimate Court of Appeal in cases of adulteration.

The volume before us treats of tea, coffee, cocoa, sugar, and honey. In the section on Tea we notice with pleasure the statement, doubtless official, that India already furnishes considerably more than one-fifth of the gross weight imported. Concerning the composition of tea we find some valuable facts. The total nitrogen present is greater than can be accounted for by the proportion of theine, which has been hitherto isolated, joined to the vegetable albumen. Hence it is fairly inferred either that the existing determinations of theine fall short of the truth, or that some other unknown nitrogenous body must be present. Gum, in the true sense of the term, though stated by some chemists to be present to the extent of 5 to 9 per cent, was found practically absent in the samples examined at Somerset House. A small quantity, *i.e.*, ½ per cent, met with in green tea, is scarcely a natural constituent of the leaf, since, if common report belie them not, the Chinese use gummy matter in making up green teas. Pectin and pectic acid are, however, naturally present. The tannin of tea seems to differ from ordinary gallotannic acid, but Mr. Bell and his associates do not give any decided account of its nature.

The illustrations of the tea-leaf and its fragments, both as seen with the naked eye and magnifying power, are decidedly good. Indeed we must here record our satisfaction with what may be termed the botanico-microscopic portion of the work.

On the score of adulteration the author is certainly no alarmist. He gives it as his opinion that the teas found in commerce, and supplied to the public, have undergone a great change for the better. Quartz and foreign leaves are seldom present to the same extent as formerly, and foreign colouring-matters are neither applied in the same quantity nor of such varied kinds as heretofore. There is nevertheless a reference to the fact that large quantities of spurious tea, such as the notorious "Maloo mixture," have found their way into this country at no very distant date.

As regards Coffee the picture is less satisfactory. We learn that in spite of the increase of population, and till lately of a free expenditure of money, the consumption of coffee has declined during the last thirty years by five million pounds. This is doubtless due to the manner in which chicory has been foisted, or almost forced, upon the consumer. We must own that we have never been able to see any honest reason for the permission—due to Viscount Halifax, formerly known as Sir Charles Wood—to sell coffee mixed with chicory. The consumer cannot be benefitted, since, if he really prefers chicory, he can buy it separately, and mix it with coffee in any proportion he may wish. The chicory-growing farmer, who, if not a tenant of Lord Halifax,

may rent land from some friend and neighbour of that nobleman, can have no interest in the matter, unless by means of this permission the nation is induced to swallow more chicory than they would do wittingly. Nor can the grocer be the gainer, unless he can, by dint of the scheme, sell chicory at a very much higher figure than it is fairly worth. We cannot help here calling attention to the now almost universal malpractice of selling ground coffee and "a portion of the finest chicory" in tins, under the mendacious name "Coffee as in France." Surely the most gullible of our countrymen ought to know that in France coffee is sold unground, and to large consumers even unroasted!

From the volume before us we feel justified in expressing a confidence that the entire work, when completed, will be an important addition to the literature of the adulteration question.

A Manual of Practical Assaying. By JOHN MITCHELL, F.C.S. Edited by WILLIAM CROOKES, F.R.S. Fifth Edition. London: Longmans and Co.

THE art of assaying, since it was first practised in our mining and metallurgical centres, and even since the first appearance of the work before us, has undergone very decisive changes in accordance with the increasing demands made upon it. It has been greatly improved in precision and accuracy, and has laid aside in a great measure its rule-of-thumb character. A century ago the assayer's methods might yield results mutually comparable so long as they were applied to ores or furnace products of a known and unvarying composition. But in case of some novel and unsuspected ingredient being present—a very possible contingency—the operator was at fault. Even where no exceptional element occurred, the figures obtained were merely somewhat wide approximations to the truth. Thus in the Cornish copper assay the results obtained fall short of the truth not by a constant quantity, but in a ratio which, though affected by other circumstances, increases generally inversely to the proportion of copper present. The smelter gets more metal out of the ore than the assay shows.

The present, like the last, edition of the work has been most carefully revised and in part re-written by Mr. W. Crookes, F.R.S., who has made a special study of the chemistry of metallurgy, and who has enriched the work with such recently discovered methods as have been found in practice accurate and convenient. Matter that might fairly be considered out of date has been eliminated, and the modern cheminal terminology has been introduced.

As regards the assay of the precious metals great hopes were

entertained that it might be simply and rapidly effected by means of the spectroscope. The careful and prolonged researches of Prof. Chandler Roberts, F.R.S., Chemist to Her Majesty's Mint, and of Mr. A. E. Outerbridge, Assistant in the United States Mint, prove that for the present these anticipations were unfounded. Mr. Capel has, indeed, proved that the 1-4000th part of a milligramme of gold will show its characteristic spectrum if the spark is passed through the weak solution of the pure metal. But on operating on an alloy containing 3·8 per cent of gold, the remainder being silver and copper, the spectra of the two latter metals were alone visible. An alloy of copper with from 20 to 25 per cent of gold was tried, and even here the gold spectrum was barely visible. Yet if gold is alloyed with even 1 per cent of copper the spectrum of the baser metal is perfectly distinct. In an alloy of copper with even 20 per cent of nickel the latter cannot be distinguished by the spectroscope. Hence the above-mentioned authorities both conclude that when two or more metals are jointly present the spark can, to some extent, elect as its medium or vehicle the one which is most readily volatilised, Hence extreme sensibility is not a universal characteristic of the spectrum reactions.

There is another practical difficulty not readily to be overcome. The alloys of gold used in the arts are not, and cannot be made, absolutely homogeneous. Two portions, each of 10, or 5, or 2 grains, might give results exactly corresponding; but the minute particles of metal which are volatilised, and which give the result in spectroscopic analysis, cannot be assumed to be safe representatives of the entire ingot.

The work before us has, in the general opinion of practical men, no rival in English literature, and we have every confidence that the present edition will be welcomed as an improvement on the foregoing.

The Herring and the Herring Fishery, with Chapters on Fishes and Fishing, and our Sea Fisheries in the Future. By J. W. De Caux. London: Hamilton, Adams, and Co.

This little book contains such a diversity of matter that the most varied tastes may be gratified by its perusal. As regards the portion which more particularly appeals to our attention, the author almost at the outset exposes an erroneous classification of fishes of which some naturalists are guilty, *i.e.*, a division into such as live at the bottom of the sea, such as invariably live in mid-water, and such as invariably live at or near the surface. He denies the existence of the surface swimmers as a class. The herring and mackerel, types of this group, are oftener

fathoms below the surface and are not uncommonly even caught in trawling nets. Mr. De Caux admits, from this point of view, only two great classes—ground fishes and swimming fishes. The former, with rare exceptions, are flat, tenacious of life, and after death less perishable than the free swimmers. Hence is drawn the practical conclusion that free-swimmers like herrings, mackerel, mullets, &c., should be eaten as fresh as possible, whilst soles, turbot, &c., to a certain degree, improve with keeping like game.

The author doubts the alleged power of fishes to support very high and very low temperatures with impunity. Jesse tells us of a gold-fish frozen up in a solid block of ice, and yet recovering its liveliness when the ice was thawed. But Jesse is a very questionable authority, as Waterton declared in quite plain language. In speaking of the specific gravity of the water in different parts of the globe the author makes a remarkable statement. He tells us that "the water of the Baltic is denser than that of the North Sea, and the water of the Dead Sea is denser still." Now all authorities agree that the water of the Baltic is merely brackish, containing less salt than that of the Ocean. Hence it should be not denser but lighter.

On the question whether fishes sleep or not he is undecided. Recent observations at the Berlin Aquarium seem to have decided this question in the affirmative. He denies them the sense of hearing. Nevertheless it must be admitted that the essential portion of the hearing organ is present in most fishes, though in none is there any direct communication between this organ and the water outside the animal. Mr. De Caux asserts that "sound cannot be produced beneath the surface of the sea." So far is this from being the case that the rate of transmission of sound produced under water has been measured, and found to be about four times as rapid as through air.

The author does not think fishes devoid of feeling, and pronounces angling for "sport" to be a cruel and barbarous amusement, the chief seat of the sense of touch being in the mouth and surrounding parts. This, by the way, is a subject for anti-vivisectionist anglers to ponder over! The rubbish often found in the stomachs of cod, &c., he believes is not swallowed by the fish but is thrust down after its death by the fishmonger to weight it.

The periodical migration of the herring he disproves. Herrings do not travel far from the spot where they are spawned as appears from the fact that certain varieties are always found in the same localities. Herrings spawn wherever they happen to be, whether in deep water, over a sand-bank, or near the shore; they spawn at least twice a year, if not all the year round, and the spawn developes not only at the bottom but near the surface. These propositions are fully proved by the facts here narrated.

Into the commercial, statisical, and historical portions of this little work, whilst fully conceding their importance, we are not prepared to enter. We note merely the author's conviction that the sea is not inexhaustible, and that without proper regulations the supply of fish, as far as British waters are concerned, may come to an end.

Transactions and Proceedings of the Royal Society of Victoria. Vol. XVII. Melbourne: Mason, Firth, and McCutcheon. London: Williams and Norgate.

We are very sorry to have to pronounce this volume most unsatisfactory as issued by the principal scientific body in a country like the colony of Victoria, where an almost infinite amount of work remains to be done. From the Presidential address we learn that there are already recorded as natives of Australia 7000 dicotyledonous plants, 1600 monocotyledons, and 1900 acotyledons. Sixty species of *Eucalyptus* have already been described and figured by Baron Mueller in a work which he has in progress. It is satisfactory to learn that a Field Naturalists' Club has been formed which may do some of the work which the Royal Society neglects. The Melbourne Museum of Natural History now numbers upwards of 44,000 specimens completely classed and labelled.

The most interesting paper in the Transactions is an account of the Hodgkinson Gold Field, Northern Queensland, hy the Rev. J. E. Tenison-Woods, F.L.S., &c., which gives a geological, botanical, and physiographical sketch of a little-known region.

Mr. P. H. MacGillivray, M.R.C.S., describes two new genera of polyzoa, *Diplopora* and *Densipora*.

"Additions to the Lichen Flora of Queensland," by Dr. James Stirton, F.L.S., is an account of upwards of thirty new species, several of which belong to new genera. This paper, as well as a subsequent one by the Rev. J. C. Tenison-Woods, on some new marine mollusca, is open to a grave objection. The respective authors have followed the very stale and unmeaning practice of giving the descriptions in Latin.

To us, at least, it seems curious to find in the Transactions of a Royal Society 18 pages occupied with a paper on "Proportional Representation," by a Prof. E. J. Manson. Its proper sphere would have been a political debating club. Two papers on "Recent Improvements in Electric Lighting," and on the "Tay Bridge Catastrophe," are merely accounts of well-known facts and not records of research and observation. In short, to any lover of Science the volume before us cannot be other than a disappointment. The President, indeed, speaks hopefully, and notes the "acquisition of new and energetic working members who have taken up investigation and research." But as to any results the volume before us is silent.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

PROFESSOR BALL'S "GLIMPSE THROUGH THE CORRIDORS OF TIME."

To the Editor of the Journal of Science.

SIR,—In a lecture delivered in Birmingham, Prof. Ball, Astronomer Royal for Ireland, recently announced his discovery, by mathematical processes, that the Moon is the child of the Earth. At first, he says, the Moon revolved round its primary very near to it, but in the course of countless millions of ages it has gradually receded from it, and is still receding. This, I need hardly remind your readers, is the conclusion arrived at in my book on "The Constitution of the Earth," * and which has been further expounded in several articles in your pages. (See "Journal of Science" for August, 1880, May, 1881, and June, 1881.) "At the time of its birth," says Prof. Ball, "the Earth was not as we see it now, clothed with vegetation and teeming with animal life. It was a huge inorganic mass, too hot for life, perhaps hot enough to be soft or viscid, if not actually molten. The offspring was what might be expected from such a parent. It was also a rude inorganic mass. Time has wrought wondrous changes in both parent and child. Time has transformed the Earth into an abode of organic life. It has transformed the Earth's offspring into our silvery Moon." The Professor then goes on to describe the way in which these changes have been brought about, in which he declares that he is guided "by a light which cannot deceive. It is the light of mathematical reasoning."

Mr. G. H. Darwin has been making the phenomena of tides a special study, and he has arrived at the conclusion that the solid earth itself is in constant motion, as the result of tidal influence. The Sun produces tides as well as the Moon, but the solar tides

* The Constitution of the Earth; being an Interpretation of the Laws of God in Nature by which the Earth and its Organic Life have been derived from the Sun by a Progressive Development. London: George Bell and Sons.

are so small compared with the lunar tides that we have hitherto overlooked them. Reasoning mathematically, Prof. Ball concludes that, before the Moon existed, the Earth was disturbed by the solar tides alone. These primitive tides were manifested as throbs in the actual body of the Earth itself, which was then in a more or less fluid condition. Though at this remote epoch the solar tides were small, as they are at present, a succession of impulses can produce a great effect. At first the vibrations were small "but at each succeeding impulse the amplitude was augmented, until at length the cohesion of the molten matter could no longer resist: a separation took place; one portion consolidated to form our present Earth; the other portion consolidated to form the Moon."

Thus, according to Prof. Ball, the Earth and the Moon were created. I have not in my book ventured to enter into such minute detail, but I am exceedingly glad to find that the Astronomer Royal for Ireland does not see any mathematical objection to my theory of the Moon's receding motion.

The Moon, having thus assumed an independent existence, began to recede very slowly from the Earth, and Prof. Ball thinks that, at the time when the earliest stratified rocks were being laid down, it may have been 40,000 miles away from it. "On the primitive oceans the Moon raised tides as it does at present; but the 40,000-mile Moon was a far more efficient tide-producer than our 240,000-mile Moon. The nearer the Moon the greater the tide. To express the relation accurately we say that the efficiency of the Moon in producing tides varies inversely as the cube of the distance. . . . If the present tides be 3 feet, and if the early tides be 216 times their present amount, then it is plain that the ancient tides must have been 648 feet. . . . These mighty tides are the gifts which Astronomers have now made to the working machinery of the Geologist. They constitute an engine of terrific power to aid in the great work of Geology. . . . As the Moon recedes, the period which it requires for a journey round the Earth increases also. . . . Do not infer that the rate of the Earth's rotation is increasing; the contrary is the fact. The Earth's rotation is getting slower, and so is that of the Moon; but the retardation of the Moon is much greater than that of the Earth. Even though the rotation of the Earth is much more than the primitive three hours, yet that of the Moon has increased to several times that of the Earth."

Such are the discoveries which Prof. Ball has made by the light of mathematical reasoning, whilst sitting, as he says, at his desk, and as the result of diligent labours of the most abstruse kind.

The Professor does not, however, inform us by what means the Moon has reached its present distance from the Earth. He obviously believes that gravitation is an absolute force in Nature, and not, as Newton taught, a purely abstract conception of the

laws or principles of motion. Why did not that same attractive force which caused the big tides when the Moon was so much nearer the earth prevent the moon from receding? What has been the force which has raised the Moon 240,000 miles from the Earth? We have been told that the attractive force of the Sun originally lifted the Moon out of the Earth; but as, according to Prof. Ball, the Earth is eighty times as heavy as the Moon, the Sun should certainly have exercised as great an attractive force upon the one as upon the other, and therefore no further separation should have taken place. They might both have dropped into the Sun before now, but, if the attraction of two bodies varies inversely as the cube of their distance, it was impossible for them to move asunder. The theory that the Sun's attraction may have been the cause of the Moon's receding motion is, moreover, utterly inconsistent with the fact that, in one portion of her orbit, she is 240,000 miles more distant from the Sun than is the Earth. Instead of the Moon being attracted towards the Sun, in one-half of her orbit she is going in the reverse direction, and does so to the extent of 480,000 miles. The same attractive force cannot be supposed to produce opposite mechanical effects.

Prof. Ball believes that "every body in the universe is capable of producing, and actually does produce, tides in every other body." The Earth may therefore have arisen from the Sun, as the Moon has arisen from the Earth. If the Moon is the child of the Earth, reasoning from analogy, the Earth may be supposed to be the child of the Sun. Such is my conclusion. Children are in the habit of growing. I believe that the whole of the planetary bodies have been derived from the Sun by a progressive development. The evolution of species is part of a great scheme of creation in which the whole solar system is concerned, and "natural selection" is one of many methods by which organic life is brought into harmony with the Earth's perpetual constitutional change. My conclusions are founded, not upon mathematical reasoning based upon a fiction as to the power of gravitation, but upon an immense range of circumstantial evidence, supported by direct observation. Biologists and geologists both inform us that the history of the Earth has been one of progressive development. The facts of Astronomy teach the same lesson. Thus there is a relation between the sizes of the planets and their distances from the Sun; suggesting that, if they have risen from the Sun, as Prof. Ball says the Moon has risen from the Earth, they have grown as they have receded. There is a relation between the number of satellites of a planet and its distance from the Sun; again suggesting a progressive development. There is a relation between the rotary motion of a planet and its distance from the Sun, the rotary velocity increasing with the distance. Instead, therefore, of the Earth's motion of rotation having diminished, as alleged by Prof. Ball,

the facts point in the opposite direction. This conclusion is, moreover, supported by the measurements of the Earth's distance from the Sun in connection with the transit of Venus, which distance has been found greater at each succeeding measurement; and the Earth's growth is demonstrated by the cracks which are everywhere found on its surface,—such cracks as, I venture to say, can neither be satisfactorily accounted for by tides nor denudation,—and by the fact that, whenever a base line has been measured a second time at the expiration of any considerable number of years, the latest measurement has always been found the longest.—I am, &c.,

ROBERT WARD.

Newcastle-upon-Tyne, December 12, 1881.

GEOLOGICAL FAULTS: SUGGESTIONS.

To the Editor of The Journal of Science.

SIR,—Faults are one of the mysteries of Geology, and it is to make a suggestion or two that I venture to communicate with you, which perhaps might be utilised and taken advantage of by gentlemen who have ample opportunities for demonstrating, &c.

Why are faults, as often found, V-shaped? How has such a phenomenon occurred?

If these faults be examined, are they not found with one leg continued—a rough Y? This at once suggests itself as an explanation if one side totally outside the V was to slip or rise. Would it not very probably dislodge this wedge-shaped piece from off the other ridge side of the fault? Or if both moved, as is possibly unlikely, ditto? I should think it would be found, on extensive observation, that the lines of strata in such faults would be nearer on a level on one side of the V than on the other; *e.g.*, if a down throw to the left, the strata in the wedge would be nearer to the corresponding strata on the *right* side than on the left, because the second side of the wedge would not begin until the other had made some progress in order to tear it off, &c. Faults are not wholly owing to district subsidence, or more remotely to shrinkage. May not the unequal compression of underlying strata, originated by pressure, escape of gases, heat, water, &c., in those strata be the cause thereof? Because faults are decided, and long, in many cases. As to compression witness stone-packed walls, &c., in mines, &c.

It is demonstrable that faults of the same district are not all of the same period. The new formations are found lying unconformably on older formations, *each* with their separate set of

faults. And probably under these again would be other sets. I am strongly inclined, therefore, to believe it to be owing to causes ending finally in local compression, or *yielding* fluctuation of strata, waves, in a rough sense, of semi-fluidified strata.—I am, &c.,

D. Y. C.

Leeds, December 2, 1881.

PROFESSOR HUXLEY ON ANIMISTIC HYPOTHESES.

To the Editor of the Journal of Science.

Sir,—Perhaps, in the interest of Modern Medicine, you could kindly spare so much space in an early issue of the "Journal of Science" as will enable me to direct attention to a fallacy involved in an assertion of Mr. Higgins, in the "Correspondence" columns of your last number, while traversing Professor Huxley's contention in a Lecture before the late Medical Congress, "that contemporary physiological science is antagonistic to Animism." Your learned correspondent holds "that in our modes of thought and expression we cannot help being animistic, and that in Biology 'mimicry' cannot be expressed except in terms of Animism." But surely a little reflection will satisfy even Mr. Higgins himself that the very reverse is the case, and that we cannot express animistic ideas except in terms which belong to Hylozoism, the nomenclature of Immaterialism being strictly materialistic. The very term *Anima* itself, *Spirit*, *Soul*, *God*, *Ghost*, *Psyche*, *Pneuma*, *Angel*, *Heaven*, and even *Idea* (vision), have all primarily a materialistic and temporal, or physical and somatic, derivation and signification. Vain are all our attempts to escape from the *mechanism* of our anatomy; a vanity which seems, conclusively, to answer Mr. Higgins's challenge to Prof. Huxley, by giving mechanical (histological) rationalia for all so-called animistic phenomena.—I am, &c.,

Robert Lewins, M.D.

Army and Navy Club, December 5, 1881.

BUTTERFLY MIGRATIONS.

To the Editor of the Journal of Science.

SIR,—In looking over the notices of butterfly migrations I find that, in many instances, a flight of these insects seems to have been the presage of a storm coming from the same quarter. Is this a mere coincidence, or is there a causal connection? Perhaps your able contributor Mr. A. H. Swinton may be able to throw light upon this question.—I am, &c.,

CONSTANT READER.

ANIMISM *versus* HYLOZOISM.

To the Editor of the Journal of Science.

SIR,—Courtesy, no less than what I regard as the cause of truth and sound philosophy, alike demand a few concluding words from me in reply to those who thought it right to criticise the Animistic view I put forth in your pages twelve months ago; and who reaffirm their opposition (though I grant in a most fair and temperate manner) in the letter printed in your December number. Of the seventeen sentences of which that letter consists I select some five or six as containing the gist of the controversy between us:—

(1.) My motto, "God is light," is pronounced *suicidal*, being apparently taken as equivalent to saying that Light is God—a converse which is the reverse of the truth. But does C. N. really think that either St. John or myself intended to affirm that 'God is light' in a literal sense? It means one of two things, probably both: that God is the Source, Origin, and Cause of natural light; or that this element—the purest, most wonderful, and most universal of all created things—is the most suitable emblem of Him who made it. Objectively, light is the æther in vibration. This motion is due to a force or power ever acting in various modes and degrees: and I maintain that that power is the power of the Almighty—the Anima Mundi, as He has been called.

(2.) *Function*, no doubt, means *office*, *duty*, or *operation*. Then, according to C. N., *mind* is an office, duty, or operation of *matter*. But each of these terms implies a conscious agent who fulfils the duty, or performs the operation; it implies, in

fact, *mind*. It is obvious, however, from the fourth and some other sentences in this letter, that C. N. is using the word 'mind' in a different sense from that in which I employ it, viz., as signifying thought, desire, intention, &c., while I use it to indicate the *person* or *being* which thinks—the *ego*, the man himself. The distinction here referred to is as important as it is real, and the neglect of it produces endless confusion. It is fully recognised in the Latin by the difference between 'animus' and 'anima,' the former being C. N.'s sense, the latter mine. Mind in the former sense is the outcome or manifestation, or perhaps condition, of mind in the latter. But further, C. N. asserts that the brain (or its grey matter) "thinks—*ergo*, it exists." I neither dispute the existence of the brain, nor that it *thinks*, *i.e.*, that it is the organ of thought. It does not think, *as* matter, but because it is associated, for a time, with a living conscious entity. If it could think, *per se*, it should do so when separated from the rest of the body, which no sane person will affirm. God has made the brain, or its gray matter, if you please, essential to thought, and it is only in that secondary sense that it can properly be said to think. The celebrated phrase 'Cogito, ergo sum' must not be thus travestied, for the 'ego' is the living soul, and not one of the bodily organs. It has been well said on this subject, by Dr. Moore, "Thought has no analogy to any known property of matter. Could any one dare to talk, even in metaphor, of definite atoms of mind? or of a soul being divisible by weight or measure?" But there is nothing inconceivable in the idea of an union or blending of a material with an immaterial entity.

(3.) C. N. makes the marvellous assertion that "The volitions of man are . . . as purely due to physical causes as the fall of a stone." If 'due to' means 'influenced by,' no one will dispute the truth of the assertion. But more than this is intended. This idea is made by its advocates to be a ground for the denial of the freedom of man's will and of his responsibility to a higher power, and is closely allied to Fatalism. Nor will the clause "in the last analysis" save it from the charge. But the illustration upsets the thesis. For gravitation, which brings a projected stone to the solid earth, is allowed by the best philosophers to be not only an universal (as far as we know), but an *ultimate*, fact: *i.e.*, one explicable *only* by referring it to the Supreme Will, and this implies a Mind. So does human volition imply a human mind.

(4.) The 'phlogistic theory' is again produced, and declared to be parallel with the 'animistic theory,' and to 'have no *analogic* ground for supposing a levitating factor.' Without admitting the parallelism, I venture to remark that such an analogy, though no doubt an inadequate one, *does* exist. For a mass of cork or a bladder of air, attached to a stone in water, would be a 'levitating factor' to raise it to the surface.

(5.) But I gladly turn from criticising to commending. In sentence 12 C. N. alludes to "a power 'behind' organic

function which presides over the animal economy"—(I would add, 'and much more'); and says that Hylozoism is not concerned to dispute the existence of an *ubiquitous* Deity. Well, Haeckel himself admits as much as this; and Goethe much more, when (after St. Paul) he calls the Deity 'the *All-Upholder*.' But C. N. adds, *If* we assume this "we must believe that He acts in all cases directly, and not by living or lifeless mediation." This is a 'non sequitur.' If such a Being exists, He must have a *Will;* and whether He acts one way or another must depend absolutely upon that Will. It is perfectly true that He—

> "Warms in the Sun, refreshes in the breeze,
> Glows in the stars, and blossoms in the trees."

Yet none of these natural objects is *God:* they are but His beauteous works, and gifts to his sentient and rational creatures. Nor do I object to their being called 'the Epiphany,' that is 'Manifestation of God's Power.' Then let us all adore Him, love Him, and obey Him, for He is the "Father of all mercies." Such Science as this, so far from being *degrading*, is the most noble and elevating of all Sciences, and I cannot imagine that you, Sir, would deem it unsuitable to your Journal.—I am, &c.,

J. H. Barker.

NOTES.

THE "Gentleman's Magazine" gives the following "strange story":—"While the subject of ghosts is attracting attention, I will offer a nut for our scientists to crack. For obvious reasons I am compelled to omit names. The wife of one of our most distinguished scientific men—I use the term 'most distinguished' advisedly, since the reputation of the man in question is cosmopolitan—saw nightly an old man seated in an arm-chair, near the fireplace in her bed-room. Being thoroughly imbued with her husband's views upon scientific subjects, she held her peace, and tried with partial success to convince herself that it was a delusion. Somewhat later this room was converted into a night nursery, and ultimately into a spare bed-room, with the result that each successive occupant, juvenile or of mature years, described the curious old gentleman who came and sat by the fire. My scientific friend has 'pished' and 'pshawed' at these statements, and has treated the whole matter as ridiculous. He has, however, been compelled to concede something to the vision or the delusion, and to quit the house."

Our attention has been drawn to an able letter on the inconsistency of Anti-Vivisectionists, which appears in the "National Reformer." We make a significant extract:—"A year or two ago some men were charged with skinning frogs alive, and then putting salt on them to see which frog could be made to jump the farthest. As this was done for sport, and for a wager, the magistrate had to dismiss the case, remarking that had such actions been done for any scientific purpose a severe penalty could have been inflicted."

Not wishing to be unjust to any body of men, we must modify the opinion we have expressed that the Free-thinkers of England were, as a whole, pledged to the Anti-Vivisectionist movement, and consequently to be ranked as enemies of Science.

The disease among peach-trees known as the "Yellows" is gradually spreading in the United States, and without due care it will be imported into Europe.

Mr. W. H. Edwards ("American Naturalist") questions the assertion of Mr. Scudder that *Danais Archippus* is single-brooded over all North America, and continues on the wing through the season, laying eggs all the time. He, on the contrary, finds that there are three or four broods yearly in West Virginia, and that the females which have hybernated die in May, after having deposited their eggs.

It appears that no beautiful or useful organic species, animal or vegetable, becomes naturalised in any country without human intervention, whilst the noisome and the ugly contrive to extend their range in spite of man's efforts to the contrary.

The "American Naturalist" records a peculiar bellows-like structure of the anthers of *Rhexia Virginica*, by means of which the pollen is actually blown out upon the body of a visiting insect.

M. H. Viallanes ("Comptes Rendus") communicates very interesting results on the post-embryonic development of the Muscidæ. The tissues of the larva are destroyed before the adult animal is formed, and at a certain point of time the pupa has a true embryonic character. The entire skin of the body is destroyed by the degeneration of the hypodermic cellules, so that the animal is merely bounded by a thin cuticle. On cutting open the abdomen of a pupa of from two to four days, we find the body composed merely of two layers of cellules; the one forming a solid chain composed of the epithelial cellules of the digestive canal reverted to the embryonic state, and the other at the circumference formed by embryonic cellules derived from the muscular nuclei and the cellules of the adipose tissue.

According to Mr. Francis Darwin the phenomenon of circumnutation can be distinctly traced in the *Phycomyces*.

Mr. A. Tylor ("Geological Magazine") calculates that the denudation of the land by rivers and the sea, with the present rainfall, is equal to one foot in a thousand years, and in the Pluvial period ten times as great. Hence the deltas of all our great rivers are later than Post-pliocene, and of the age of the Pluvial period.

Mr. Clement Reid, F.G.S. ("Geological Magazine") combats the views of Mr. Howorth as to the sudden extinction of the mammoth in Siberia. He considers it evident that the mammaliferous deposits in that country cannot have been frozen all at once, or through radiation from the present surface, as there are numerous sheets of clear ice, which must have been successively formed, interstratified with the ice.

Dr. Merrill ("Forest and Stream") states that, according to the belief of certain Indian tribes, certain small birds effect their migrations upon the backs of larger ones, such as the sandhill crane, the white crane, and the Canada goose.

Mr. J. A. Allen ("Scribner's Magazine") gives the following conclusions on the migrations of birds:—That the habit of migration resulted from changes of climate, occurring at a not very remote geological period; that every gradation occurs between species the most widely roving and such as are stationary, and that even members of the same species may be either migratory

or stationary, according as they occupy for breeding stations the northern or southern portion of the common habitat; failure of food induces a return towards warmer regions (this is not the case with the swift, which leaves England in August, when its food is plentiful); that the return to the breeding station is prompted by the recurrence of the breeding season; that they pursue certain routes guided by inherited experience, and that they discern approaching meteorological changes.

H. D. Minot ("American Naturalist") makes some very interesting notes on the migration of birds. Thus in case of some species a single pioneer bird has been known to arrive about a week sooner than his fellows, and to depart southwards again. The most favourable haunts are the first revisited. The general character of a season has a great influence on the time of arrival, whilst temporary heat or cold has little save upon water-fowl. The order of species is uncertain, even amongst near allies.

According to Mr. W. E. D. Scott, of the Nuttall Ornithological Club ("American Naturalist") migratory birds, when travelling by night, fly at elevations of from 1 mile to 4 miles above the earth's surface, and in clear nights can thus recognise the leading features of a very wide sweep of country.

The Anti-Vivisection craze will find no countenance in France. The new Minister of Education, M. Paul Bert, is himself a man of Science.

The "Revue Spirite" and "Light" give the following incident under the heading "A Pathological Phenomenon":—"A girl of sixteen has been ill here for eight months. For the last twenty days she has been without food or substantial liquid. During the latter time she predicted that at a certain hour or a day she would have a fit, and would be thought dying; but that at a certain hour she would wake up and ask for food. The prediction proved true. When she woke she was ravenously hungry, and ate without inconvenience. Her abdomen had been in her illness tympanitically distended. Part of her prediction was that the distention would disappear, and it did very rapidly. From the time of her leaving off food she spoke several languages of which she knows nothing in her ordinary state; knew persons whom she had not seen before, and what they had about them; knew their thoughts; knew those who talked of her, and what they said; and putting her hand upon an open book she knew the page and contents of it; the time of day without the clock, &c. She had not been magnetised." This narrative is dated Campan, Upper Pyrenees, August 28th, 1881, and is signed by D. Adorret.

According to the "Boston Journal of Chemistry" a "malarial wave" is extending over New England.

The same journal revives the tradition of spider's web, or preferably an entire live spider, being a permanent cure for ague.

Prof. A. B. Prescott, in a paper read before the American Association for the Advancement of Science, shows the limited biological importance of the organic syntheses effected by chemists. He adds that all the matter of protoplasm is in a state of chemical combination, but that it cannot therefore be accepted that chemical composition supplies the essential conditions or impulses for organisation or other vital functions. The synthesis of all the chemical compounds of the living body may or may not be attainable in the laboratory, but its success would give no whit of promise for the development of organisation. Chemical action is distinct from cell organisation as it is from heat, cohesion, &c., and its correlations with all these forces have to await demonstration by experiment. Cell growth appears to be a necessary factor in the simple splitting of sugar into alcohol and carbon dioxide, and it may or may not be an essential factor in the chemical synthesis of proteids or of cellulose.

M. L. Crié, in a communication to the Academy of Sciences, gives an account of phosphorescence in a number of cryptogamous plants, such as *Agaricus olearius, igneus, noctilucens, Gardneri, lampas*, &c.; *Auricularia phosphorea* and *Polyporus citrinus; Rhizomorpha subterranea* and *setiformis*, &c.

M. E. Yung ("Comptes Rendus") has observed that the diet of tadpoles has a remarkable influence upon the relative numbers of the two sexes.

M. A. Gaudry has laid before the Academy of Sciences an account of a deposit of the bones of the reindeer discovered near Paris. He gives the following sketch of the history of the quaternary epoch in the Paris basin :—1. Hot phase : deposits of St. Prest; *Elephas meridionalis*, transition from the tertiary to the quaternary period. 2. Great glacial phase : deposit on Montreuil; herds of reindeer; *Rhinocerus tichorhinus*. 3. Hot phase : diluvium at the foot of Montreuil; *Hippopotami, Rhinocerus Merkii, Elephas antiquus;* fig-trees and laurels of La Celle? 4. Temperate phase : diluvium of low levels of Grenelle; mixture of tropical and arctic forms. 5. Brief return of cold : reindeer; the rhinoceros disappears. 6. Present climate : epoch of polished stone.

M. P. Bert, the new French Minister of Education, has communicated to the Academy of Sciences an important paper on the limits of safety in the surgical use of anæsthetics. His researches necessarily involved experiments which in England would be branded as vivisection.

M. E. Grimaux ("Comptes Rendus") has succeeded in the synthesis of a body resembling in many of its reactions the proteine compounds. His point of departure, however, was of an organic nature.

A singular meteorite fell at 3.35 p.m., March 14th, 1881, about 1¾ miles from Middlesborough, and is described by Prof. Herschel in the "Monthly Notices" of the Royal Astronomical Society,

According to M. F. Hément ("Comptes Rendus") children deaf from birth, and who have been taught to speak at the Pereira Institution, acquire the peculiar accent of their native district. M. Blanchard calls this observation strongly in question.

M. A. Certes ("Comptes Rendus") shows that the ova of certain low crustaceans, especially *Artemia salina*, can bear with impunity desiccation for months, and even for years.

M. H. Toussaint, in a paper read before the Academy of Sciences, supports the contagious character of tubercular disease.

We note with regret that a medical contemporary actually proposes that University professorships should be won, not *rebus gestis*, but by competitive examination!

Dr. Noerdlinger ("Central Blatt für Forst-Wesen"), as the result of prolonged observation, denies that the quality of timber is in the least influenced by the age of the moon at the time of felling.

According to the experiments of E. and H. Becquerel, conducted in the Jardin des Plantes in the winter of 1879-80 ("Œsterr. Landwirth. Wochenblatt"), a thick layer of snow does not protect the subjacent soil from falling below freezing-point. Grass-turf is a more efficient protection.

M. Duponchel ("Comptes Rendus") traces an agreement between the curve of the solar spots and the actions resulting from the eccentric movement of the large planets.

M. P. Laur ("Comptes Rendus") finds that under certain circumstances electric currents may be produced by solar light.

Dr. Manuel da Gama Lobo, of Rio Janeiro, is said to have discovered the toxic agent in yellow fever to be derived from a "species of Infusoria, the *Opunsia Mexicana*, which belongs to the family of *bacillæ*." (We fear there is here some error; bacillæ are certainly not Infusoria.)

"Science" gives an account of a meeting of the New York Medico-Legal Society when a paper on Alcoholic Trance was read by Dr. Crothers. The discussion which followed took a very personal form.

Prof. Winchell, of the University of Michigan, in a Lecture delivered before the Summer School of Philosophy, at Greenwood

Lake, declared that "Evolution is the name of a mode of continuance, not of a mode of beginning. It can neither affirm nor deny any mode of origin."

An extensive collection of British birds, contained in two hundred cases, has been recently presented to the Preston Museum by Mr. J. B. Hodgkinson.

In the "Transactions of the Edinburgh Geological Society" Mr. T. Stock gives an interesting account of certain Natural History collections which he inspected during a tour on the Continent in 1880. We note the following very just reflection :—"A public museum rarely has money enough to purchase the larger collections as they appear from time to time in the market, and the fact of its being known to possess a revenue at all (the amount of it is exaggerated in popular estimation) has the effect of stinting private beneficence." He considers that "the best museums of Natural History in Britain are rather in advance of than behind the majority of Continental institutions of the same rank."

The volume of the "Journal of the Royal Microscopical Society" for 1881 has just been issued. There is no reason in any way to alter the opinion already given respecting the merits of the work. It is certainly unique among societies' journals, as it not only publishes the papers read before it, and reports of the meetings, but it gives an account, often at considerable length, of the progress of every branch of science in any way relating to the microscope. The papers read before the Society have during the past year been only thirteen in number. At first sight it would seem remarkable that an influential society should have the original contributions to its Transactions fewer in number than they were ten or twenty years ago; but it must be borne in mind that papers which formerly came before the Microscopical Society exclusively are now received by many other societies, although from the manner in which the observations have been carried out they would quite as justly come within the province of the Royal Microscopical Society. There has, however, been no dearth of matter, for owing to the energy of the Editors the volume contains more than a thousand pages, and gives a very full account of nearly everything that has transpired in microscopical matters all over the world.